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CIVIL ENGINEERING.

Report on Decay of the Stone at the New Palace at Westminster.

From the London Builder, No. 974.

THE report of the committee appointed by the First Commissioner of Her Majesty's Works and Public Buildings, to inquire into the decay of the stone of the New Palace at Westminster, and into the best means for preserving the stone from further injury, with the minutes of evidence, has been published as a "Return to an Order of the Honorable the House of Commons, dated the 1st of August, 1861," and can be obtained by the public in the usual way. The committee met thirteen times (between March 23d and August 7th), irrespective of the meetings of sub-committees; examined thirty-one witnesses; and considered seventy-seven communications submitted to them. The following, addressed to the Right Hon. Wm. Cowper, M. P., is their

REPORT:

Sir:—We, the undersigned, being the committee appointed "to inquire into the decay of the stone of the New Palace of Westminster, and into the best means of preserving the stone from further injury," have the honor to submit to you the following report, in which we have adopted, as the objects of our inquiry, the several points referred to by your letter of appointment and instruction,* viz:—

"I. The extent and position of the decay.

* The letter of appointment and instruction was addressed:—"To Sir Roderick Impey Murchison, G.C.St.S., D.C.L., LL.D., F.R.S., Director-General of the Geological Survey of Great Britain; William Tite, Esq., M.P., F.R.S., President of the Royal Institute of British Architects; Sydney Smirke, Esq., Royal Academician, Architect; George Gilbert Scott, Esq., Royal Academician, Architect; Geo. Godwin, Esq., F.R.S., and Matthew Digby Wyatt, Esq., Vice Presidents of the Royal Institute of British Architects; Augustus William Hofmann, Esq., LL.D., F.R.S., Professor of the Royal College of Chemistry; Edward Frankland,

"II. The causes to which it is attributable, taking into consideration the composition of the stone, and the influence exerted upon it by moisture, and by the acids diffused in the London atmosphere.

"III. The best means of preserving the stone from further injury."

"IV. The qualities of the stones to be recommended for future use in public buildings to be erected in London."

1. In proceeding with the important inquiry thus entrusted to this committee, we beg to state that we first made a careful inspection of the whole of the buildings; and that after this inspection we proceeded to obtain such evidence as appeared to us best calculated to facilitate the inquiry entrusted to us, by examining a considerable number of witnesses who had been connected with the building from the commencement, or who had been concerned in the various processes which had been actually tried for arresting the decay which had occurred; and also another class of witnesses, who had suggested various theoretical remedies for the same purpose.

2. We delegated to a sub-committee, specially appointed, an examination and inquiry into the condition of other buildings erected in the metropolis, in which magnesian limestone had been used; and we particularly called the attention of the scientific chemists, who had been appointed on the committee, to several points peculiarly within the limits of their acquaintance with chemical subjects.

3. We also considered it expedient to invite by advertisement the attention of chemists and others to the subject submitted to the committee; requesting that any plan or suggestion for the prevention of decay, or for arresting its progress, might be brought under our notice.

4. Having thus premised the course the committee thought it expedient and desirable to take, we now proceed to report *seriatim* on the subjects brought under our notice by your instructions, and in the order adopted therein.

I. The Extent and Position of the Decay.

5. It is extremely difficult to give any very exact account either of the extent or actual position of the decay. It seems from the evidence that it first began to make its appearance in the portions of the Palace at Westminster executed at the commencement of the building about seven years after their execution; and yet, in some of the most recently executed portions, viz: those towards Old Palace-yard, facing Henry VII.'s Chapel, the decay appears to be as obvious as in any other part of the building.

6. In the earlier works, viz: those towards the Thames, the decay is most apparent in the lower portion of the building; and in this portion the decay is confined to what may be called "zones," or general levels; which would seem to suggest that it depends as much upon po-

Esq., F.R.S., Professor of Chemistry at St. Bartholomew's Hospital; Frederick Augustus Abel, Esq., F.R.S., Chemist to the War Department; David Thomas Ansted, Esq., M.A., F.R.S., Professor of Geology; James Tennant, Esq., Professor of Geology at King's College, London; George Rowdon Burnell, Esq., Civil Engineer; Thomas Hawksley, Esq., Civil Engineer; Charles Harriott Smith, Esq., one of "the Commissioners for the selection of the stone for building the Houses of Parliament;" and Edward Middleton Barry, Esq., Associate of the Royal Academy, and Architect in charge of the houses of Parliament; the committee appointed by the First Commissioner of Her Majesty's Works and Public Buildings to inquire into the decay of the stone of the New Palace at Westminster, and into the best means for preserving the stone from further injury." Mr. Alfred Bonham-Carter acted efficiently as Secretary.

sition in the building as upon the use of particular beds of stone from the quarries employed.

7. The same remark applies to the part of the palace fronting the approaches to Westminster Bridge, where the decay of the lower portion is considerable; but, in the newest work, facing Henry VII.'s Chapel, the decay occurs in positions which are more varied, and under circumstances which it is exceedingly difficult to appreciate.

8. We have examined with much care the upper portions of the building; and we cannot perceive that the decay has made any important inroad upon those much more exposed portions, where decay might more reasonably have been expected. The decay, however, occurs again to a considerable extent in the inner courts, which are sheltered in a great measure from external influences; and, perhaps, the very worst specimen we have noticed is to be found in the small archway leading to the reporters' gallery, near the entrance to Westminster Hall; a part of the work as much sheltered as in the nature and circumstances of a public building it could well be.

9. The general result of our observations, confirmed by the evidence, would seem to suggest that the stone used in the Palace of Westminster is much more likely to decay in damp and sheltered situations than where it is exposed to the full action of atmospheric influences. In the east and north fronts, before adverted to, the worst symptoms occur in the ashlar between the upper and lower mouldings of the plinth, and under the first cornice, where the exposure is inconsiderable; but the dampness, arising from the drip of the mouldings and from the action of capillary attraction, in cases where projections hold the moisture, appears to exercise an important influence on the condition of the stone itself.

10. It does not appear to us that the decay is attributable, as is commonly supposed, to the stones in the building not being placed upon what is technically called their natural bed, or in the same relative position as they occupied in the quarry: thus, stones which are found horizontally in the quarry appear to have been often placed perpendicularly in the building, and used for purposes of the most delicate decoration without any injurious result. As an instance of this fact, we may point out the elaborately carved shields of arms under the range of the first-floor windows: the stones used for these shields, though universally placed perpendicularly to their natural position in the quarry, present, so far as we are aware, few, if any, symptoms of decay.

11. The extent to which the decay on the whole surface has proceeded, it is not very easy to estimate. At the present moment the actual decay is, doubtless, considerable for a building so recently erected; but the change of color in the stone itself, and the "fretting out of the surface," which are suggested as the first symptoms, lead us to apprehend that there may exist much more mischief than at present is actually apparent.

12. One of the witnesses examined, however, and whose judgment as a practical man is of considerable value, is of opinion that the decay,

after proceeding to some depth in the stone, stops of itself; that an induration of the surface takes place, and that no further decay ensues. The committee would willingly accept this opinion, if they considered it well founded; but they cannot conceive that it is true to any considerable extent, notwithstanding there may certainly be some few indications which lead to the belief that in some cases it may be correct.

13. At present the decay appears for the most part on the plain surfaces, whilst the finer and more elaborately-wrought portions of the building, unless under projections, are not seriously affected. And, however disappointing and disfiguring these defects may be, especially in a building so recently erected, the committee are of opinion that at present the decay does not affect the stability of the structure.

II. The Causes to which the Decay is Attributable.

14. This part of the inquiry naturally leads to a reference to the evidence which has been obtained by the committee on the subject of the stone itself. The result of this evidence may be thus briefly stated. The stone recommended by the commissioners for this building was that from the quarries of Bolsover Moor and its neighborhood; and this stone was actually contracted for in the first instance. Before the work began, however, it was found that blocks of sufficient size could not be procured from those quarries; and in consequence, one of the commissioners was appointed to proceed to the spot, to ascertain whether other quarries might not be discovered furnishing stone in beds of greater thickness and of larger dimensions. These conditions were found in the quarries of Anston, and the stone of greater thickness procured from these quarries has been used not only in this building, but in all the other buildings constructed of magnesian limestone in the metropolis, after the quarries of Bolsover Moor had been abandoned, for the reason above stated.

15. The recommendation of the Bolsover stone in the report of the commissioners was founded on its similarity to that used in the Norman portions of Southwell Minster, which were stated in the report to be in a high state of preservation. Evidence has since been adduced, in a letter from Mr. Scott, to be found in the Appendix, which renders it probable that the stone of this Minster was really obtained from the ancient quarries at Mansfield Woodhouse. The latter quarries were re-opened, and a considerable quantity of stone from them (exceeding 20,000 cubic feet) was made use of in the Palace of Westminster; but in their turn they were relinquished, from dissatisfaction as to the size of the blocks, though we have it on evidence, confirmed by our own observation, that the stone used from these quarries has stood remarkably well.

16. The evidence brought before your committee on the subject of the stone obtained from the Anston quarries is very conflicting; the contractor and his principal foreman stating that the stone was, with slight exceptions, extraordinarily good; while other witnesses maintain that even in the quarries themselves there are stones in a state of ac-

tual decomposition; and one very important witness, a foreman employed at the Palace at Westminster, asserts he knew that certain beds in some of the quarries were liable to decay, and that he abandoned them in consequence. With reference to the selection of stone, the committee venture to remark, that it is much to be regretted that the offer made by one of the commissioners, particularly well acquainted with the selection and working of stone, to examine that used in the Palace at Westminster for the moderate salary of £150 per annum, was not accepted; owing to some difficulty in regard to the party who was to be held responsible for this unimportant amount; and that the matter was left to persons who admit they had little or no prior experience of this description of stone, though they evidently entertained suspicions of the durability of some of it which they were employing.

17. With reference to the very natural and important question of the actual causes of the decay of this stone when exposed to the London atmosphere, the committee take the liberty of referring to the report of the chemists, who were members of the committee, to whom this question was specially referred. This will be found in the Appendix.

III. The Best Means of Preserving the Stone from further Injury.

18. This part of the inquiry referred to the committee naturally divides itself into two questions; namely, as to the steps that have hitherto been taken, whether experimentally or otherwise; and as to those that are to be recommended for adoption hereafter. With regard to the first question, we have ourselves examined with care the result of what has been done at the Palace itself, either experimentally on the river front, or, as in the inner courts, by actual coatings or washings over large surfaces. With regard to the second question, our inquiries have been earnest and elaborate, and we have examined many witnesses and given much time to the consideration of the various propositions obtained by advertisement or otherwise. As will be seen in a subsequent part of the report, we finally referred this question to the further consideration of the professional chemists who were on the committee.

19. On the first question, the committee are decidedly of opinion that it is not necessary nor desirable to proceed with any general coating, painting, oiling, or washing of the whole building. It is quite obvious, in their judgment, that a very large proportion of the stone does not require any such application; but that what is wanted is some efficient process which should be applied to the surface of any stone that begins to show symptoms of decay, with a view to arrest its progress. The committee believe, that the persons to whom the care of the building is entrusted ought to watch it, and note, in the very earliest stages, wherever decay is perceptible, by efflorescence, change of color, crumbling, or slight decomposition.

20. In cases where the decay is important, and evidently occasioned

by the fall of rain on an upper projecting or exposed surface, protection should be afforded by a covering of sheet zinc or lead; and if, hereafter, any composition should fortunately be discovered, by which the decaying stone could be at once covered or coated, and the injurious influences of the atmosphere prevented from further acting upon it, the difficulty would be solved. In some extreme cases, the decayed stone might be cut out, and replaced with a new one. With regard to the processes which have actually been applied, whether experimentally or extensively, your committee are decidedly of opinion that the discovery of a proper mode of treating stones in a state of decay has not yet been made; and there is no evidence whatever on the building itself to induce them to believe that the decay, where decay has arisen, has been arrested, or that permanently the decay has been prevented, by any of the processes applied.

21. With reference to the second question, we found ourselves unable, after much labor, to come to any definite conclusion; and we finally requested the chemists in the committee to examine and report upon it; but those gentlemen state, as appears by their report in the Appendix, that the nature of the inquiry is so extensive, and that time is so important an element in the solution, that they are unable to give any opinion upon the subject. They further state, that they spent five whole days in the examination of only one suggested remedy; but they are unable, notwithstanding, to give any opinion on even that one suggestion. They allude to secret processes, regarding which they say they can offer no opinion; but they express a doubt of the applicability of any suggestion which would demand the veil of secresy for protection. Concurring in this view, it may be further noted that even if such applications were found successful in sample or experiment, no security would be afforded for a corresponding success in any subsequent large operations. They recommend that a series of experiments should be conducted, under chemical supervision, for a considerable period of time; and the committee are most reluctantly compelled to coincide with them, and to urge upon the government the adoption of such a course.

IV. The Qualities of the Stones to be recommended for future Use in Public Buildings to be erected in London.

22. On this head of the inquiry the committee have been unable, in the time allotted to them, to go into any very extensive examination. It is obvious, however, that although some varieties of magnesian limestone are an excellent and durable material, when not exposed to the deleterious influences of the London atmosphere; yet that in London it is subject to causes of decay, which render it an undesirable and unsafe material for the construction of public buildings.

23. It is equally obvious that Portland stone, well selected, has been used in buildings in London from the date of St. Paul's downwards, under circumstances of great exposure, and with most successful results. Portland stone is a material to be obtained in any quantity, and in blocks of any size, beautiful in color and texture, reason-

able in price, not by any means so hard as the Anston stone, and yet with a power of resisting the influences of the London atmosphere, that leaves but little to be desired. It must be remarked, however, that Portland stone should be carefully selected; an operation which would be the most satisfactorily effected by an agent at the quarries.

24. On this subject the commissioners could of course bring much personal experience to bear; but, after the valuable explanation of the principles upon which the decay of stone depends in populous places, as given by the chemists, in their report before referred to, the committee refrain from repeating those conclusions; in which, however, they entirely concur.

25. During the inquiries of the committee, one of their members, Mr. Burnell, who is well acquainted with architectural and engineering works in France, undertook, at his own expense, a journey to Paris, to inquire into the practice of the French architects engaged in the government works in that metropolis. There, the stone used, the "calcaire grossier," though a carbonate of lime of tertiary age, and therefore of very different mineral composition from our magnesian limestone of the much older Permian age, seems to suffer also from decay in a comparatively pure atmosphere, and where wood is chiefly used as fuel.

26. From the evidence of Mr. Burnell, it does not appear that French architects or chemists have been more successful than ourselves, either in the use of materials not subject to atmospheric influences, or in the application of processes for arresting decay when it has once begun. The opinions of the most scientific chemists and architects in France on this subject have, however, in this way been obtained; and it is extremely probable that the inquiries undertaken by them, simultaneously with those undertaken in this country, may hereafter lead to some successful result.

27. The committee have to thank the government for the facilities given to Mr. Burnell in this important part of the inquiry, by providing him with an introduction which obtained for him the active assistance of her Majesty's ambassador at the court of the Tuilleries.

28. The committee delegated, as before stated, to a sub-committee, the duty of examining the various buildings in London in which magnesian limestone from the Anston quarries had been introduced in the external architecture. The report of this sub-committee forms part of the Appendix; and we beg to refer to that report as confirmatory of our opinion of the uncertain character of magnesian limestone, and the risk attending the use of it in London.

29. In conclusion, the committee venture to recommend that the architect of the Palace of Westminster, assisted by scientific chemists, should examine and record the actual state of the stone work of the building at the present moment; that experiments should be made by their direction, under various conditions of height, exposure, and aspect, with such preservative materials and agents as the chemists may suggest from time to time; and that researches should be continued into the effects of the various alkaline silicates, the phosphates,

and other substances which have been brought under the notice of the committee, or suggested in Germany, France, or elsewhere; that where decay arises from damp, means should be taken to protect the stone, as has been before suggested; that any stone extensively decayed should be removed and replaced; but that in particular the earliest symptoms of decay should be carefully watched, and examined, with the view to the application of some immediate remedy. The committee believe that a very large portion of the stone in the Palace of Westminster is of a very durable nature; and they entertain a confident expectation that a remedy will soon be found to arrest or control the decay when it has unfortunately begun to appear.

William Tite,	M. Digby Wyatt,	George R. Burnell,
Rod. I. Murchison,	A. W. Hofmann,	Thomas Hawksley,
Sidney Smirke,	E. Frankland,	Charles H. Smith,
Geo. Gilbert Scott,	F. A. Abel,	Edward M. Barry.
George Godwin,	James Tennant,	

Alfred Bonham-Carter, Secretary.

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Report of Sub-Committee of Chemists, referred to in the foregoing: and addressed to Chairman of the Committee.

17th June, 1861.

Sir:—We have the honor to inform you that we have complied with the wishes of the committee, by examining into the several proposals which have been laid before them for the preservation of the stone of the New Houses of Parliament; and that we have arrived at the following conclusions:—

1. Amongst the processes proposed, varying in principle and value to a very considerable extent, there is not one which we at present feel justified in proposing that the committee should definitely recommend as a preservative, either for general or local application.

2. A minute examination into one class of processes, submitted to the committee at an early period, has convinced us that, surrounded with great difficulties as the subject appeared at the outset, the obstacles eventually met with in an effective experimental inquiry are of a far more formidable character than could have been anticipated. Having devoted five days exclusively to the practical study of one of those processes (Ransome's), and having been unable in that period to elaborate even this single process sufficiently to warrant us in expressing a definite opinion upon its merits, it is obvious that anything like an elaborate examination of the numerous proposals which have only just now been submitted to us would require the expenditure of a far greater amount of time than the committee could place at our disposal.

3. Whilst regretting that it is not in our power to lay before the committee a positive recommendation of any particular process, we beg to submit the following observations:—

An examination into the nature of the several processes proposed, leads to their classification under two heads:—

(*a*) Processes which are likely to afford permanent protection to the stone.

(*b*) Processes which are only calculated to afford protection of a temporary character.

In both of these classes there are proposals which may at once be excluded from further consideration, on account either of their inapplicability to stones when placed in a building, or of the obvious misapprehension, on the part of the proposers, of the problem to be solved.

A proposal to protect stones by immersion in a boiling mixture of pitch, or resin, and oil, may be quoted in illustration of the processes which are only applicable to stones previous to their having become integral parts of any structure; again, the suggestion to cover the building with a coating of a mixture of silica with sulphur, applied in a semi-fluid condition, would involve almost insurmountable difficulties in its practical application; not to speak of the inflammability of the sulphur, which is only slightly diminished by the presence of the silica; or the uncertainty of the temporary character of the protection which, under the most favorable circumstances, could be afforded by this material.

Several of the suggestions are based upon notions so obviously erroneous, such as coating the building with sulphate of lead, and procuring an alleged galvanic protection by establishing connexions of this coating with plates of zinc; or, of ridding the building of the principle of decay by fermentation, that no object whatever could be gained by entering more fully into the merits of these proposals.

Of the processes which are intended to afford permanent protection to the stone, and the use of which is not precluded by the conditions of the case, there are several which claim a careful investigation. These processes may be classed under the following heads:—

1. Application of silicates of the alkalies, in various states of concentration.

2. Application of silicates, in conjunction with various saline compounds, intended to produce double decomposition.

3. Application of hydrofluoric or hydrofluo-silicic acid, or their saline compounds.

4. Application of phosphoric acid, and acid phosphates.

5. Application of solutions of the alkaline earths, or their bicarbonates, in water.

All these processes are more or less based upon chemical considerations, which are supported by analogy, and which, in the case of the two first-named classes, have received considerable experimental confirmation. The experiments which are now in progress with several of the processes included in the first two subdivisions, will, we believe, in the course of a few years, furnish ample data for correct conclusions regarding their applicability. In the meantime, it might be advisable to apply to portions of the New Houses of Parliament actually undergoing decay, certain processes selected as representatives of the remaining classes above enumerated, in order that their merits might be

submitted to the only conclusive tests,—those of actual application, and protracted exposure to the corrosive influence of a London atmosphere.

The second division of processes, namely, those which are only calculated to afford protection of a temporary character, are, from their very nature, of minor importance for the purposes of the committee's inquiry; nevertheless, as the claims to permanence of none of the processes of the first division have as yet been substantiated by the test of time, we would recommend that, in addition to the experiments already made in this direction, further trials be instituted of some of the more promising materials of this particular description. This recommendation is based upon the consideration that substances included under the appellation of organic, differ essentially in their powers of resisting the destructive action of the atmosphere. Whoever is acquainted with the nature of organic substances, cannot fail to appreciate the different degrees of stability under atmospheric influence exhibited by gluten, gelatine, or starch (which we find enumerated among the proposed protective agents), and by bees-wax and paraffine, not to speak of many of the fossil gums, which exhibit a degree of permanence approaching that of mineral substances.

The materials which we would recommend for selection to be tried in comparison with linseed oil, are paraffine, bees-wax, and some of the more permanent gums and resins, applied in the form of solutions in volatile solvents.

We should not omit to remark, that some of the witnesses and others who have addressed the committee, speak of secret processes. We cannot, of course, offer any opinion regarding such proposals; but we should doubt the applicability of any suggestion which would demand the veil of secresy for protection.

Finally, we beg to state, as the result of the experience which we have been enabled to acquire during the prosecution of our investigations on this subject, that a definite solution of the question at issue can only be arrived at after the lapse of a considerable period; since the relative merits of the processes which we recommend for trial can be established only by the test of time.

A. W. Hofmann,
E. Frankland,
F. A. Abel.

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Report of Sub-Committee on Nature and Causes of Decay of Building Stones.

17th July, 1861.

Sir:—Having been requested to submit to the committee our opinion on the nature and causes of the decay of building stones generally, and of the stone employed in the construction of the New Houses of Parliament in particular, we now have the honor to submit the following observations:—

Building stones in general may be divided into two classes:—

1. Those which consist of materials not easily acted upon by acids.
2. Those composed of materials which are, partially or entirely, acted upon by acids with facility.

As an illustration of the first class, granite, porphyries, and serpentines may be quoted; whilst to the second belong limestones, dolomites, and certain sandstones, containing carbonate of lime as cementing material.

The stone used in the New Houses of Parliament belongs to the second class of building materials, consisting, as it does, almost entirely of the carbonates of lime and magnesia. The following analyses of several varieties of dolomite by Professor Daniell and Messrs. T. Ransome and B. Cooper, are quoted in illustration of the general composition of the stone in question:—*

	Bolsover Moor.		North Anston.		Woodhouse	Steetley.
	Daniell.	Ransome & Cooper.	Ransome & Cooper.	Ransome & Cooper.	Ransome & Cooper.	Ransome & Cooper.
				Stone ends		
Carbonate of lime,	51·1	52·07	54·89	55·37	52·80	53·95
Carbonate of magnesia,	40·2	40·60	42·07	41·71	44·31	43·78
Sulphate of lime,						trace.
Protoxide of iron,		0·89	0·49	0·73	0·63	0·64
Oxide of iron and alumina,	1·8					
Peroxide of iron,		0·83	0·24			
					Carbonate.	
Protoxide of manganese,		trace.	trace.	1·68	1·84	
Silica,	3·6	3·64	0·56	0·92	0·47	0·44
Water,	3·3	0·48	0·51	0·45	0·23	0·12

	Roach Abbey.	Huddlestone.	Park Moor	Lindley's Bolsover Quarry, Woodhouse, near Mansfield.
	Daniell.	Daniell.	Daniell.	Ransome & Cooper.
Carbonate of lime,	57·5	54·19	55·7	54·05
Carbonate of magnesia,	39·4	41·37	41·6	38·58
Protoxide of iron,				0·74
Peroxide of iron,				0·62
Oxide of iron and alumina,	0·7	0·30	0·4	
Carbonate of manganese,				2·43
Silica,	0·8	2·53	0·0	1·30
Water,	1·6	1·61	2·3	0·46

Regarded from a purely chemical point of view, the difference in the resisting power to corrosive agents of different stones, would appear, at first sight, to depend entirely upon their chemical composition; but even a moderate acquaintance with the properties of the components of such building stones, demonstrates that there are other conditions at least equally instrumental in determining the degree of permanence of different stones.

It is a well-established fact that *the same* chemical substance exhibits, in different conditions, a great variation in its behavior with chemical agents. Numerous examples might be quoted in illustration of this. Thus, marble and chalk are chemically identical; but, owing

* The analyses by Daniell are quoted from the "Report of the Inquiry undertaken under the Authority of the Lords Commissioners of Her Majesty's Treasury, by C. Barry, Esq., H. T. De la Beche, Esq., W. Smith, Esq., and Mr. Charles Smith, with reference to the selection of stones for building the New Houses of Parliament." Those by Ransome and Cooper are extracted from a paper on "The Composition of Limestones used for Building Purposes, especially on those employed in the Erection of the New Houses of Parliament," contained in vol. ii., part ii. (1848), of the Memoirs of the Museum of Practical Geology.

to the difference in their physical structure, the one being crystalline and the other amorphous, the former is much less readily acted upon by acids than the latter. Again, artificial peroxide of iron is readily soluble in acids; peroxide of iron, in the form of hæmatite, is attacked with difficulty by acids; and the same oxide, after exposure to a powerful heat, is almost entirely insoluble in acids. The influence of aggregation in these instances, and in numerous others which might be quoted, is obvious, and generally admitted by chemists, however different and imperfect may be their views regarding the connexion between physical condition and chemical effect.

The observations just made regarding the behavior of substances such as enter into the composition of building stones, cannot but apply with equal force to the aggregates of such components to the building stones themselves.

The atmospheric influences to which building stones are subject, are many of them essentially chemical actions, involving processes analogous to, or identical with, those performed in the laboratory; although, from the extreme dilution of the chemical agents, as existing in the atmosphere, they must necessarily be of a very gradual character.

There are few instances in which the influence of the state of aggregation upon the permanence of a building stone is more apparent than in that of the dolomitic limestone, used in the construction of the New Houses of Parliament. Here, in one and the same block of stone of comparatively small dimensions, we find certain portions of the surface powerfully disintegrated, while others appear in a perfectly sound condition. Chemical analysis has hitherto failed to establish any important difference in the composition of sound portions of such stones and those parts which are subject to decay: it is, therefore, legitimate to attribute the unequal permanence of the stone, under atmospheric influences, to such structural differences as may be comprehended under the term—state of aggregation.

Before proceeding to an examination of the particular character of the decay observed in the stones of the New Houses of Parliament, it may perhaps be desirable to glance at the nature of the changes to which building stones generally are subject under atmospheric influences. Under normal conditions, these changes must be ascribed to the action of the oxygen, carbonic acid, nitric acid, and water, in the atmosphere. In the air of towns, however, there are certain other constituents, such as several acids of sulphur, and occasionally hydrochloric acid, which cannot fail to exert an additional disintegrating influence upon building stones.

The action of oxygen must be of comparatively a subordinate character; its effects being confined to constituents which occur but rarely, and generally in limited proportions, in building stones; such as the sulphides of iron, and the protoxides of iron and manganese; these compounds, being very prone to oxidation, would tend to disintegrate the stones by the absorption of oxygen. Of far greater importance are the effects of carbonic acid and water. Carbonic acid, in

the presence of water, is a powerful solvent: it not only corrodes the calcareous and magnesian carbonates (more or less powerfully according to their state of aggregation), whether they form the principal constituents of the stone, or are only present as cementing materials; but is capable even of attacking and gradually decomposing the hardest and most indestructible rocks.

In the case of the calcareous and magnesian constituents of stones, carbonic acid acts by transforming the insoluble earthy carbonates into soluble bi-carbonates, which are thus removed from the substance of the stone; whilst its influence on silicious rocks consists in the elimination of the alkaline bases, in the form of carbonates, and the separation of the silica in a more or less friable condition. The weathering of granites, and their gradual transformation into the several varieties of porcelain clay, afford an interesting illustration of the latter kind of action. In the changes just mentioned, the carbonic acid and water are equally concerned; the water serving not only as a vehicle for the introduction of the carbonic acid into the pores of the stone, but also as a solvent for the products of its action. There are changes, however, to which building stones are subject, in which water is the sole agent, and which are more of a mechanical than of a chemical character. The expansion which water undergoes on freezing, and the irresistible force which it then exerts, are well known: it is obvious that water freezing within the pores of a stone must exercise a disintegrating action not less powerful than those above referred to.

Recent researches have demonstrated that nitric acid is a frequent and perhaps even a normal constituent of the atmosphere, and, as such, must undoubtedly assist in the destruction of magnesian and calcareous stones; but the proportions in which this acid has been found are so minute, that it need not be dwelt upon as an important destructive agent. This remark, however, does not apply to the acids referred to above, as existing in the atmosphere of towns. The quantity of sulphur-acids in the air of towns where a considerable amount of coal is consumed, is quite appreciable. According to the determinations of Dr. Angus Smith, the air of Manchester contains an average proportion, corresponding to one part of sulphuric acid in every 100,000 parts of air, which, in the centre of the town, rises to twenty-five parts in 100,000. No numerical data exist with regard to the proportion of sulphur-acids in the London atmosphere; but it can scarcely be doubted that, in the neighborhood of the New Houses of Parliament, they are present to an extent equal to the average amount found in the Manchester air: they must, therefore, be regarded as among the more important agents, destructive to stone, which are present in the London atmosphere.

A few observations remain to be offered regarding the particular nature of the decay manifesting itself in some of the stone of the New Houses of Parliament. It has already been pointed out that, so far as our experience goes, we are inclined to attribute the local character of the decay to structural differences, obtaining in different parts of the stone. The general structure and the composition of the

stone in the New Houses of Parliament render it, moreover, amenable to all the sources of disintegration which we have above enumerated, with the exception, perhaps, of oxygen, which can scarcely produce any appreciable alteration in dolomite. Thus, the chemical action of carbonic and sulphuric acids, in combination with water, will gradually dissolve and remove the carbonates of lime and magnesia, whilst the porous nature of the stone renders it liable to the mechanical effects of water under the influence of frost. The presence of sulphuric acid in the air of towns appears, in the case of magnesian limestone, to bring into play another process of destruction. This acid not only corrodes and renders soluble, as we have pointed out, the earthy carbonates (in which respect it resembles carbonic acid in its effects), but, forming with magnesia a readily crystallizable salt, the well-known sulphate of magnesia, remarkable for the large proportion of water of crystallization which it fixes, it gives rise, in addition, to a mechanical destruction of the stone, precisely similar to that produced by freezing water. The powerful mechanical effects resulting from the solidification of water, induced by crystallization, are well known; although it would appear that they have not hitherto been sufficiently appreciated as auxiliaries in the process of disintegration of stone. The analogy between the solidification of water by freezing and by crystallization, is perfectly obvious; and a French chemist has suggested, as a means of recognising stones liable to disintegration by frost, to immerse them in a solution of sulphate of soda, and to note the subsequent effects of its crystallization within the stone.

We have ourselves recently had occasion to observe some phenomena which go far to elucidate these destructive effects of crystallization. The exfoliations exhibited by many of the fictile vases deposited in the British Museum were found to be due to the formation and crystallization, within the substance of the vessels, of nitrate of lime. Again, in experiments on the preservation of fabrics by impregnation with saline substances, it was found that the crystallization of sulphate of magnesia, within the material, produced a disintegrating effect upon the fibres, sufficient greatly to weaken the material.

In conclusion, we would remark, that the effect attributed to the crystallization of the sulphate of magnesia in assisting the decay of dolomitic stones, and more particularly of those used in the construction of the New Houses of Parliament, is borne out by the existence of a marked efflorescence of sulphate of magnesia upon those portions of the stone where exfoliation has taken place.

A. W. Hofmann,
E. Frankland,
F. A. Abel.

Railway Capital.—The annual return made to the Board of Trade shows that at the end of the year 1860, of the total capital raised by the railway companies of the United Kingdom, namely, £348,130,127, 54·8 per cent. had been raised by ordinary shares, 19·5 by preference shares, 2·2 by debenture stock, and 23·5 by loans, the respective amounts being £190,791,067, £67,873,840, £7,576,874, and £81,888,546.

Railway Stations.

Nouvelles Annales de la Construction.

A large portion of the September number of this periodical is taken up by illustrations of the railway stations erecting and about to be erected on the line of railway now in course of construction from Ancona to Bologna. These stations have already been referred to in the *Annales*, and now a report, pointing out the principle upon which they have been arranged, is furnished, illustrated by plans and other drawings, and worth the attention of those who have the arrangement of such buildings under their control.

Four classes of stations have been designed by Messrs. Oppermann for this line of railway, and we extract part of the descriptive report accompanying them, as useful in pointing out the way in which the exigencies of traffic should be taken into account, by the engineer and the architect, in laying down the first lines even of the plan of a railway station. It is almost needless to repeat the observation which we have already had occasion to make more than once, viz: that Continental railway management differs in so many details from English, that it is impossible for a Continental railway station ever to furnish a perfectly serviceable model for an English one. It would be well, however, if even only as much forethought, and as timely a recollection of simple and very obvious considerations, had been brought to bear upon some of our English stations, as is shown in the accompanying memorandum, and in the four simple plans which illustrate it.

"*Memorandum*.—In the general arrangement of the buildings on the line from Ancona to Bologna, the attempt has been made, so far as practicable, to satisfy the following conditions:—

1. A separation of the passenger and goods departments: waiting rooms, refreshment rooms, conveniencies, and telegraph office being towards the right; baggage room, goods offices, and porters, &c., rooms on the left.

2. Making the distance which each traveler has to go over between the times of his entering and leaving the station, as short as possible.

3. Arranging the rooms in an order corresponding with the usual positions of 1st, 2d, and 3d class carriages (1st and 2d classes at the head of the train, and 3d class behind).

4. Avoiding any confusion of streams of travelers coming in with those going away, by arranging that the exit from the station shall not open upon the entrance lobby.

5. The above precaution is equally desirable in the case of baggage. In 1st class and 2d class stations, baggage is received through one door, and given out through a different one.

6. The station-master's office is in immediate contact with all the departments which he has to superintend (booking office, baggage office, postal and telegraph offices, &c.), and near the staircase which leads up to his dwelling.

7. The refreshment rooms are at one end of the station adjoining the waiting rooms, and the conveniences at the same end, detached.

8. In fact, a general correspondence has been established between the positions of the departments of the station, and the order of the carriages, &c., in the train (as standing opposite the platform). The lamp and oil room is opposite the engine, the baggage office opposite the baggage wagons, the 1st and 2d class waiting rooms near the head of the train, and the 3d class near its tail.

9. It is practicable to convert one class of stations into another, by the simple addition or suppression of bays or lengths; to facilitate this, the buildings are made to terminate at either end with a plain gable.

10. On the first floor all the rooms are separate, approached from one corridor lighted from its extremity, so that the rooms can be appropriated in any way desired. A kitchen and conveniences are placed at the head of the staircase."

The report, which gives some further details, also observes that it has been judged advantageous to place the refreshment rooms and the conveniences both at the same end of the station, and away from the other offices, as by this arrangement passengers actually in the train do not get confused among the passengers entering it; and the guard, should there be absentees at the moment when he requires to start, has only to go in one direction to look for them. The convenience is also very justly pointed out of having all the parts of each station, whatever its rank and consequent size, arranged in similar relation to each other; the traveler thus always knows beforehand, as if by instinct, in any station on the line, to what part he must go for any particular department.—*Civ. Eng. and Arch. Jour.*, Oct., 1861.

Railway Traveling.

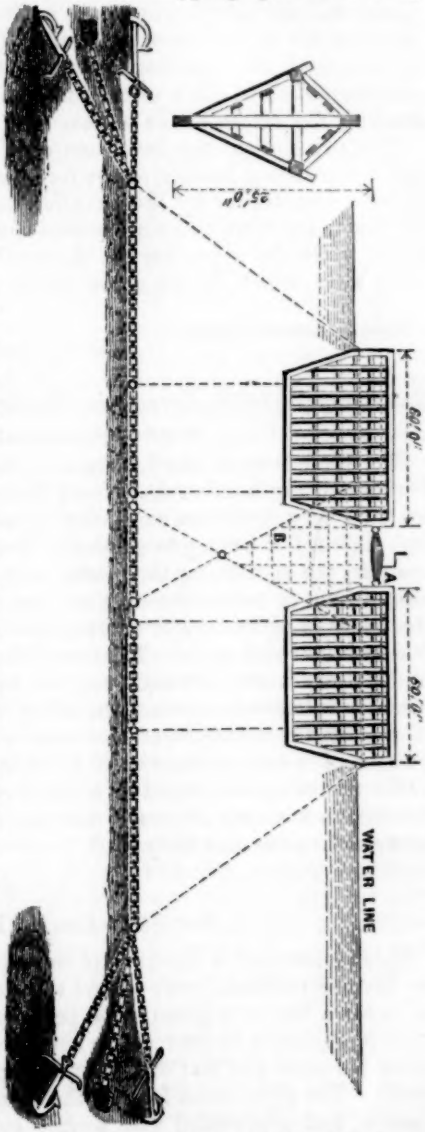
From the Lond. Mechanics' Mag., September, 1861.

The number of travelers by railway in the United Kingdom last year was 163,435,678, besides 47,894 holders of season and periodical tickets, who must have made very many journeys; in the whole there must have been much nearer six than five journeys in the year for every soul in the kingdom. The trains, passenger and goods trains together, traveled 102,243,692 miles, which is further than going 4000 times round the world; 267,134 horses and 357,474 dogs made railway journeys, little to their liking. The goods traffic comprised 12,083,503 cattle, sheep, and pigs, and 89,857,719 tons of minerals and general merchandise. In these vast piles of property conveyed from place to place, the minerals double the general merchandise in quantity, and they are carried at a little more than a quarter of the cost; 60,386,788 tons of minerals produced to the railway companies only £4,951,899, while 29,470,931 tons of general merchandise brought them £9,157,987. The receipts of the railways (10,433 miles in length at the close of the year) from all sources of traffic were £27,766,622, of which £13,085,756 came from passenger traffic and the mails, and the residue from goods. The expenditure was £13,187,368, or 47 per centum, leaving rather more than £14,500,000 net receipts. The compensation paid for accidents and losses amounted to £181,170. The quantity of rolling stock was no less than 5801 locomotives, 15,076 passenger engines, and 180,574 wagons for goods traffic, in all 201,451 engines and carriages. The numbers are enormous, and they are enormously increasing. Comparing last year with the year before, notwithstanding the bad weather, the passengers increased by 13,600,000, the minerals by 8,600,000 tons, the receipts by above £2,000,000, the miles traveled by trains by nearly 9,000,000. 3,896,960 trains ran in the course of the year 1860, upwards of 10,000 a-day, or more in a day than seven times the number of minutes in the day.—*Times.*

Admiral Tayler's Breakwater.

From the Lond. Engineer, No. 247.

SIR:—The formation of breakwaters and harbors of refuge, for the purpose of affording better shelter to our shipping upon our coasts, and of a less costly description, and better adapted for certain localities, than the system of solid masonry hitherto in use, and upon which millions of the public money is now being lavished, has been debated upon in Parliament, several inventions have been submitted to the government, and it is now recommended, by the select committee appointed to inquire into the practicability of such breakwaters and harbors, that £10,000 should be expended by government in testing any plans which may offer a probability of important results in future saving of money.



I beg to forward you a sketch of a plan for a floating breakwater, invented by Vice-Admiral J. N. Tayler, C. B., and which has been submitted recently to the government: and its adoption ultimately, I make no doubt, will be certain, from the fact of its capabilities having been already tested, and proved to be in every respect practically correct, and well adapted to the purpose intended, Admiral Tayler having devoted many years in bringing his invention to perfection.

This novel and much-approved plan consists of a number of sections of open timber framing, as per diagram, framed and bolted together, forming a structure prismatical in form; each section is 60 ft. in length 20 ft. beam, and floating 18 ft. below and 8 ft. above water mark. They are secured in position by means of ground moor-

ing chains and anchors, to which are attached bridle chains passing up through the keel to the horizontal chains, which are run through each section on both sides, forming one continual line of double chain through the entire length of the breakwater, and passing down at each extremity to the ground mooring. The sections are placed 20 feet apart, and the intervening space is filled up by netting chains, also secured to the ground mooring chain. The sections are further secured by means of iron couplings spanning the space between them, and so constructed as to allow of easy motion in the rise and fall of the sections by the action of the waves.

The principle of this invention consists in rendering the element itself the resisting barrier to the forces of the sea. This is a fact proved beyond dispute, for by the peculiar construction of the timber work, the water entering therein becomes inert and forms a barrier against itself, while the upper part of the section, being free, receives the upward wave, which, breaking on the seaward face, is quickly dispersed.

J. E. Reid.

Newington, September 7th, 1860.

Supplying Water to Locomotive Tenders without Stopping the Train.

From the London Artizan, December, 1860.

Mr. Ramsbottom, the Locomotive Superintendent of the Northern Division of the London and North-Western Railway, has recently patented a very ingenious method of economizing time in railway traveling. Instead of having to gradually slow and finally stop the train, for the purpose of refilling the tender with water—involving also further loss of time to regain the original speed—by Mr. Ramsbottom's plan the water is picked up, or made to flow into the tender whilst the train is running at full speed. The contrivance, which is of a highly philosophical character, is simple and not liable to derangement. We have recently witnessed experiments, which demonstrated the practicability of raising three-fourths of the water contained in a trough about a quarter of a mile in length, 16 to 18 inches wide, and 6 inches deep, whilst traveling at a speed of about forty to fifty miles an hour. By this means a saving of nearly half an hour may be effected in a run between London and Holyhead.

A New Slope-Level. By M. Ribot.

This instrument is designed to solve practically either of the following inverse problems, viz: to find the slope per metre of a given line; or, to set a line to a given slope per metre (or per yard). The instrument is so simple as scarcely to need description. The horizontal distance between the feet or points of support is exactly one metre (or yard). The right hand foot of the figure is capable of protrusion by a screw, and is provided with a scale to measure the amount of this protrusion. When the two feet are on a level, the index is at the zero of the scale. If you want to determine a given slope, project

the foot until the instrument stands on the slope, the protrusion measured on the scale gives the slope in terms of the distance apart of the feet (metres or yards). If you want to establish a given slope, set the foot to the indicated point of the scale, and adjust your plane to the instrument. The lower bar of the instrument may be graduated so that the plummet shall read angles of slope.—*Bull. Soc. d'Encour. pour l'Indus. Nationale.*

Experiments on the Effects of Vibratory Action and Long-continued Changes of Load upon Wrought Iron Bridges and Girders.

From the Lond. Athenæum, Sept., 1861.

W. Fairbairn, President of the Association, presented a paper containing a series of "EXPERIMENTS ON THE EFFECTS OF VIBRATORY ACTION AND LONG-CONTINUED CHANGES OF LOAD UPON WROUGHT IRON BRIDGES AND GIRDERS."—He said this was a subject of great importance as affecting the construction of tubular and plate bridges, and also the lattice and trellis bridges. Fifteen years ago experiments were made which led to the construction of the Conway and Britannia tubular bridges on the Chester and Holyhead Railway, and determined the form in which such structures should be designed. Since that time some thousands of bridges had been built entirely of iron. The requirement of five tons per square inch on the part of the Board of Trade appeared to be founded on no fixed principle. It was well known that the power of resistance to strain of wrought iron depends very much upon the form in which it is combined, and unless the proportions of the parts were permanently established, the five-ton tensile strain might lead to error. For the purpose of making experiments upon the influence of vibration in causing the rupture of beams and bridges, he had constructed a small iron-plate beam of 20 feet clear span, and 16 feet deep, representing the proportion of one of the girders of the Spey Bridge, and exposed it to conditions similar to those of a bridge subject to changes of load as produced by the passage of trains, and in proportion to the heaviest rolling road. The beam was first loaded to one-fourth of its breaking weight, and it sustained a million changes of load without injury. The load was then increased to nearly one-half the breaking weight. With this weight the beam gave way after 5175 changes. It appeared, therefore, it was not safe to build bridges in which the rolling load would bear this proportion to the breaking weight. The beam was taken down and repaired, and the experiments were then renewed. The load was then reduced to two-fifths the breaking weight, and 25,900 changes of load were sustained. Lastly, the load was reduced to one-third, and the experiments were still proceeding, the beam being uninjured after 2,727,754 changes. In calculating the strain upon the area of the metal after deducting the rivet-holes, which, it must be remembered, were larger in proportion in this small beam than in bridges, he found that the beam would sustain no deterioration with strains of nearly 7½ tons to

the square inch. With ten tons to the square inch, the beam broke after 5172 changes. Now, as the limit of elasticity was reached at about 9 tons per square inch in ordinary boiler plates and bridge plates, it would appear that it is unsafe to load structures subject to a continually varying load beyond that point. Within those limits, however, there was no evidence that a deterioration of structure took place. For the present, he would advise that in all beams and girders, tubular or plain, the permanent load or weight of the girder and its platform should not, in any case, exceed one-fourth of the breaking weight; and that the remaining three-fourths should be reserved to resist the rolling load in the proportion of six to one. He earnestly directed attention to the laws which governed the resisting powers of girders exposed to transverse strains, to the best principles of uniting the joints, and, above all, to the selection of the best material, which, in the parts of the girders subject to a tensile strain, ought always to sustain a test of from 22 to 24 tons per square inch. The use of superior metal for the bottom of the girders would give an increase of from one-fifth to one-sixth in the strength. There was no economy—and he wished particularly to impress this on the Section—in the use of inferior iron for this purpose, and its employment inevitably led to a loss of character in the structure and danger to the public.

Lord Wrottesley expressed his satisfaction that Mr. Fairbairn, with that public spirit which characterized him, was continuing those experiments which the Iron Committee, of whom he (Lord Wrottesley) was chairman, had commenced, but which they were not able to continue, through the discontinuance of the Government grant.

Mr. Fairbairn said he was glad to state that the Government had acted in a more liberal spirit to himself, and had granted £150 to conduct the experiments.

Proceedings of the British Association.

MECHANICS, PHYSICS, AND CHEMISTRY.

Abstract of an Investigation of the Resistance of Ships. By W. J. Macquorn Rankine, C.E., LL.D., F.R.S.S., London and Edinburgh.

From the London Artizan, Oct., 1861.

This paper is a very brief extract of the results of an investigation of the laws of the resistance of ships, founded originally on experimental data supplied to the author by Mr. James R. Napier, in 1857, and first applied to practice in order to fix beforehand the engine power required for a ship in 1858. To state all the mathematical details of the investigation would occupy much more time than can reasonably be allotted to one paper at a meeting of the British Association; the present communication, therefore, will be limited to a general view of the nature of the theory adopted, a statement of the practical rules to which it leads for computing the power required to propel a given ship at a given speed, an abstract of some comparisons between the results of that rule and those of experiment, a statement of some

limitations to the application of the theory, and some general conclusions deduced from it.

I. General View of the Theory.

The importance of friction as one of the elements of the resistance of water to the motion of a ship has long been recognised. Colonel Beaufoy made many experiments on models, expressly to ascertain its amount. Mr. Hawksley, some years ago, proposed a formula for the resistance of vessels, consisting of three terms, of which two, representing the effect of pressure on the bow and stern, depend on the area of midship section, and the figures of the bow and stern, while the third, representing the effect of friction, is proportional to the wetted surface of the ship. Mr. Bourne, in his work on the screw propeller, mentions friction as an element of the resistance of ships, which must depend on the girth rather than on the midship section.

It is to be remarked, however, that in all previous investigations as to the friction of ships in moving through the water, the velocity of the sliding motion of the particles of water over the ships bottom has been treated as being sensibly equal to the forward velocity of the ship, and sensibly the same at every point of the ships bottom; whereas, in fact, it must be different at different points of the ships bottom, at some points less than the ships speed, at other points greater; and on an average greater than the ships speed, in a proportion which is greater, the more bluff the figure of the ship. No definite results are to be expected from any comparison of experiment with a theory which does not take account of those variations.

It is further to be remarked that the excess of the pressure of the water against the bow of a ship above its pressure against the stern is only an indirect effect of friction; for were it not for the loss of motive energy which takes place through friction, the particles of water would close behind the vessel with such speed as to exert a forward pressure exactly equal to the backward pressure of the particles of water which are forced aside at the bow.

The author was induced by these considerations to investigate the theory of the friction of the water against the bottom of a ship, taking into account the various velocities of sliding at various points, as affected by the positions of those points and by the figure of the ship; and making only the following assumption: that the agitation in the water caused by the friction on the ships bottom extends only to a layer of water which is very thin as compared with the dimensions of the ship. This assumption enables the ratio which the velocity of sliding at any point bears to the speed of the ship to be expressed as a mathematical function of the position of the point, and of the ships figure, by the aid of the general equations of fluid motion; and from that function is deduced a certain integral which expresses the work performed in overcoming friction over the whole wetted surface of the ship, while the ship advances through a given distance, such as one foot; and to that quantity of work the force required to drive the ship against the friction of the water is proportional. The mathematical investigation is tedious and voluminous, and is reserved for a detailed paper.

The exact expressions arrived at were very complex, but were easily reduced to more simple expressions, giving an approximation sufficient for the purpose in view.

Upon comparing the formula thus obtained with the indicated power of actual ships moving at known speeds, it was found that *the whole* power required to propel the ships could be accounted for by friction alone, leaving none to be accounted for by any excess of pressure at the bow above that at the stern, except such excess as is indirectly caused by the friction, and virtually comprehended in the expression for the power required to overcome friction.

II. Practical Rule for the Power Required to Propel a Ship, with Trochoidal or nearly Trochoidal Water Lines, *i. e.* Wave Lines.

The first rule obtained in a form sufficiently simple for practical use was the following:—

"*The resistance of a sharp-ended ship exceeds the resistance of a current of water of the same velocity in a channel of the same length and mean girth, by a quantity proportional to the square of the greatest breadth, divided by the square of the length of the bow and stern.*"

The *mean girth* is found by taking the mean of the girths, as measured on the "body-plan" of the vessel, of the immersed part of a series of equidistant frames or cross-section.

The algebraical expression of this rule is as follows:—

$$R = \frac{f w v^2}{2g} \cdot L G \left(1 + \frac{\pi^2 B^2}{L_1{}^2}\right) \quad . \quad . \quad . \quad (1.)$$

in which

R denotes the resistance of a vessel.
L her total length at the water-line, in feet.
L_1 the length of her bow and stern, in feet.
B her greatest breadth, in feet.
G her *mean girth* under water.
$\pi^2 = 9{\cdot}87$.
v the ships speed, in feet per second.
g the acceleration produced by gravity in a second, or 32·2 feet.
w the weight of a cubic foot of salt water, or about 64 lbs.
f a co-efficient of friction, whose value for iron ships in a clean state, as on their trial trips, is about ·0036, or nearly the same with the co-efficient of friction of water at high speeds in cast iron pipes.

The expression for the indicated horse power of the engine, deduced from the preceding formula, is as follows:—

$$\text{I. H. P.} = \frac{k R v}{550} = \frac{k f w v^3}{550 \times 2g} \cdot L G \left(1 + \frac{\pi^2 B^2}{L^2{}_1}\right) \quad . \quad . \quad (2.)$$

In which k is a co-efficient expressing the ratio of the gross indicated power to the effective power, allowing for the friction of the machinery and slip of the propeller. Its average value is about 1·6; so that kf = about ·00576 on an average for ships in a clean state.

But the most convenient formula for practice is one in which the velocity, v, is given in *nautical miles per hour*, and is as follows:—

$$\text{I. H. P.} = \frac{V^3}{C} \cdot \text{L G} \left(1 + \frac{\pi^2 B^2}{L^2_1}\right) \quad . \quad . \quad (3.)$$

c being a divisor, whose value is—

$$c = \frac{550 \times 2g}{4{\cdot}8064 \times k f w} = \frac{115}{k f} \text{ nearly} \quad . \quad (4.)$$

or if $k f = {\cdot}00576$, $c = 2000$ nearly.

This rule, with a co-efficient of resistance deduced from some experiments on previously existing vessels, was applied in 1858 to the computation of the engine-power required to propel a vessel then in course of construction (the *Admiral*); and at the trial trip of that vessel on the 11th of June, 1858, (the particulars of which, together with a copy of her body-plan, have been communicated to the Committee of the British Association on Steamship Performance,)* the actual engine-power was found to differ from the theoretically computed engine-power by less than one-fiftieth part of its amount, the computed power being 758, and the actual power 744, and that notwithstanding that the *Admiral* differed materially in her proportions from the vessels from whose performance the co-efficient of resistance had been deduced. The rule was afterwards applied with equal success to fix the required engine-power of other vessels built by Mr. J. R. Napier.

III. More Comprehensive Rule for the Power Required to Propel a Ship.

The rule in the form already given was deduced from a mathematical investigation based upon a trochoidal (or wave line) form of water-lines, and, therefore, although it could be applied with approximate accuracy to vessels approaching to that type, some doubt and difficulty arose in applying it to those which deviated widely from the trochoidal form. To obviate that difficulty, the rule was put into another form, which, while it was identical in its results with the original form for trochoidal water-lines, was more readily applicable to water-lines of other shapes. The alteration consists in this:—that instead of "*a quantity proportional to the square of the greatest breadth divided by the square of the length of the bow and stern, there is to be substituted a quantity proportional to the square of the chord of the mean angle of entrance of the water-lines,*" it being understood that the angle of entrance of a given water-line is the angle between its two tangents at opposite sides of the bow, at the points where it is *most inclined* to the keel; and that the mean value of that angle is to be taken for a series of equi-distant water-lines or horizontal sections of the vessel.

* See Report of that Committee to the Aberdeen Meeting of that Association, 1859.

The algebraical expression for the resistance now takes the following form:—

$$\text{R} = \frac{f\,w\,v^2}{2\,g} \cdot \text{L G} \left(1 + 4 \sin.^2 \frac{\theta}{2}\right) \quad . \quad . \quad (5.)$$

in which the symbols are the same with those already explained, except θ, which denotes the *mean angle of entrance* as already defined. In what follows, for brevity's sake, the quantity $4 \sin.^2 \frac{\theta}{2}$ is denoted by b^2.

The two expressions for the engine-power become respectively

$$\text{I. H. P.} = \frac{k\,f\,w\,v^3}{550 \times 2g} \cdot \text{L G} \,(1 + b^2) \quad . \quad . \quad (6.)$$

$$= \frac{\text{V}^3 \,\text{L G}\,(1 + b^2)}{\text{C}} \quad . \quad . \quad . \quad (7.)$$

The processes involved in this rule may be represented to the mind as follows:—

1. Multiply together the length (L) of the vessel at the surface of the water, and the mean girth (G) of the immersed parts of the cross sections or frames; this gives the *area of the internal surface of a channel or tube of the same length and mean girth with the vessel* (L G).

2. Increase that area in the ratio of unity, plus the square of the chord of the mean angle of entrance $(1 + b^2)$ to unity; this increase is an approximate value of the allowance indicated by theory for the obliquity of the surface of the vessel, and for the excess of the speed of sliding of the particles of water over various portions of it, above the speed of the vessel. The result of this process (L G $(1 + b^2)$) may be called the "*augmented surface.*"

3. Compute the height from which a heavy body must fall to acquire the speed of the ship $\left(\frac{v^2}{2\,g}\right)$ multiply that height by a *co-efficient of friction* (f) deduced from experiment; conceive a layer of water of the thickness resulting from the last multiplication, to be spread over an area equal to the "*augmented surface;*" the weight of that layer will be the resistance of the vessel at the given speed (R).

4. Multiply that resistance by the speed of the vessel in feet per second, and by a factor (k) ascertained by experiment, to allow for the loss of power by slip and by the friction of the engine and propeller; the result will be the power or mechanical energy expended in a second, which, divided by 550, gives *indicated horse power.*

Although in most cases the co-efficient of friction (f) and factor for loss of power (k) cannot be separately ascertained, their product ($k\,f$) can always be ascertained by experiment, and this may be called the "*gross co-efficient of resistance.*"

5. The more convenient rule for finding the required indicated horse power may be thus expressed:—multiply the "*augmented sur-*

face" by the cube of the speed in knots, and divide by a divisor which is found by experiment.

$$\left(c = \frac{115}{k\,f}\right)$$

IV. Comparison of the Theory with the Experiment.

In applying this theory to experimental data, the proper course is to compute from those data the value in each case either of the *gross co-efficient of resistance* ($k\,f$), or of the *divisor* (c), which is inversely proportioned to that co-efficient; and should those values present such variations only as can be accounted for by ordinary variations in the efficiency of engines and propellers, and in the condition of the vessel's bottom, the inference is in favor of the soundness of the theory. It is necessary in every case to have access to the plans of the vessel, and hence complete sets of data are less abundant than could be wished. The formulæ to be employed are as follows:—

For the divisor,

$$c = \frac{V^3\,L\,G\,(1+b^2)}{I.\,H.\,P.} \quad . \quad . \quad . \quad (8.)$$

For the gross co-efficient of resistance,

$$k\,f = \frac{115}{c} \quad . \quad . \quad . \quad . \quad . \quad (9.)$$

The following table gives nine examples of such calculations. Three of them are founded on experiments made by Mr. J. R. Napier and the author, on published data relative to Government vessels, and two are published reports of trial trips of vessels belonging to the Peninsular and Oriental Steam Navigation Company.

Example.	Length.	Breadth.	Mean draft.	Midship section.	Displacement.	Mean girth.			Augmented surface.	Speed knots.		Divisor.	Co-efficient of gross resistance.
	L.					G.	L. G.	$1+b^2$.	L. G $(1+b^2)$	V.	I.H.P	C.	
	ft.	ft.	feet.	sq. ft.	tons.	feet.	sq. ft.		sq. ft.				$k.f.$
I. Vulcan (paddle),	160	16	4·5	56	140	14·75	2360	1·1	2596	14·5	412	19210	·00599
II. Black Swan, now Ganges (s.)	244	36·5	13·8	385	1670	40	9760	1·2	11712	12	970	20864	·00551
III. Admiral (paddle),	210	32	7·5	214	820	31·5	6615	1·36	9000	11·9	744	20385	·00565
IV. Rattler (screw),	178	33	11·25	274	870	32·5	5785	1·4	8099	10·07	428	19360	·00593
V. Rattler (screw),	178	33	13·50	330	1078	37·5	6675	1·4	9345	9·64	437	19370	·00593
VI. Fairy (screw).	140	21·1	4·83	71·5	168	19·0	2660	1·2	3192	13·33	364	20770	·00554
VII. Fairy (screw),	140	21·1	5·83	82	196	21·5	3010	1·23	3702	11·9	321	19435	·00592
VIII. Ceylon (screw),	280	41	18·5	649	3000*	52·4	15196	1·16	17625	13·34	2054	20371	·00565
IX. Nubia (screw),	280	39·5	17·25	515	2100*	48·2	13496	1·16	15655	12·15	1422	19725	·00583
					*Nearly.								

The breadth, mean draft of water, midship section, and displacement, are given in each case, to show the variety of forms and sizes to which the calculations relate. The displacement ranges from 140 to 3000 tons; the proportion of length to breadth, from $5\frac{1}{2}$ to 10; the proportion of breadth to draft of water, from $2\frac{1}{2}$ to $4\frac{1}{2}$.

The final results show the *divisor* as ranging from 19210 to 20864, and the *gross co-efficient of resistance*, from ·00599 to ·00551.

V. Limitations to the Theory.

The theory stated in this paper is not applicable to vessels which are so bluff at the bow and stern as to push before them or drag behind them a mass of water full of whirling eddies; for in such vessels the assumption already stated, that the water agitated by friction is a very thin layer, is not fulfilled.

Neither is the theory applicable to a vessel which raises a wave that buries a considerable portion of her bows. This does not occur in well-shaped vessels of the sizes to which the experiments already quoted relate; but it may occur in models, as experiments made by Mr. J. R. Napier and the author have shown. Small wooden models of vessels were made, of very various proportions, the proportion of length to breadth ranging from *five* to *ten*. The *proportionate* resistance of these models when dragged in pairs at equal speeds were tested by means of suitable apparatus, and it was found that when the speed was so small as not to raise a wave exceeding the ordinary proportion of the height of the wave to the dimensions of the vessel in large ships (say from $\frac{1}{10}$th to $\frac{1}{20}$th of the draft of water), the results of the experiments exactly agreed with the theory; but when the speed was increased until the wave buried from one-half to the whole of the bows of the models, the resistance of the broader model was increased in a greater proportion than that of the narrower.

From the result of these experiments it follows that, in order that conclusions drawn from experiments on models may be applicable to actual ships, care should be taken not to move the model at a speed which raises a wave exceeding in proportionate height the wave raised by the large vessel; and, that such may be the case, the velocities of the model and of the ship should be proportional to the square roots of their linear dimensions. For example, the models already mentioned were about $\frac{1}{10\,0}$th part of the linear dimensions of the vessel that they were intended to represent; and when dragged at $\frac{1}{10}$th of the speed of those vessels, or less, their resistance followed the same laws, but not otherwise. This conclusion is common to the theory of the present paper, and to Mr. Scott Russell's wave theory.

The effect of such waves as have been here referred to on the resistance might be taken into account by means of a supplementary theory, provided a sufficient number of experiments had been made on the large scale to determine the necessary data; but in the experiments on the large scale quoted in this paper, the resistance due to the wave at the bow seems to have been insensible, or to have been balanced, or nearly balanced, by the pressure of the wave at the stern. This balanced action is to be expected in vessels whose lengths, as prescribed by Mr. Scott Russell, are equal, or nearly equal, to the lengths of waves traveling with the same speed.

VI. Deductions from the Theory.

The approximate expression for the resistance may be divided into two terms, one of which is increased, and the other diminished, by increase of length. For a vessel of a given size and type there is some

proportion of length to breadth which makes the resistance a minimum. To determine that proportion exactly by the method of maxima and minima would be a process of extreme complexity and difficulty; but from a series of approximate calculations made by way of trial, it would appear to be not very far from that of 7 to 1—a conclusion in accordance with that which some authorities on ship-building have deduced from practical experience. It appears further, that of two vessels which deviate equally in opposite directions from the best proportion, the larger has less resistance than the shorter; this conclusion also agrees with practical experience.

If, as the comparison of the theory with experiment seems to show, the resistance of a vessel is proportional to what has been called the *augmented surface*, it is the area so designated, and not the midship section, which should regulate the areas of paddles and screws.

The results of the investigation described in the paper tend to prove that friction constitutes the most important part, if not the whole, of the resistance of ships that are well shaped for speed, and that its amount can be deduced with great precision from the figure of the ship by the aid of proper mathematical processes. On this, as well as other accounts, it is to be desired that the data which are collected by the Committee of the British Association on Steam Ship Performance should be accompanied as far as possible by drawings of the ships lines; at all events, by the "body plans," from which the forms of water-lines can easily be constructed when the distances between the frames are known.

Prevention of Rotting of Wood.

To prevent posts and piles from rotting, the following coating has been recommended, which is the more suitable since it is economical, impermeable to water, and nearly as hard as stone.

Take 50 parts of rosin, 40 of finely-powdered chalk, 300 parts (or less) of fine white sharp sand, 4 parts of linseed oil, 1 part of native red oxide of copper, and 1 part of sulphuric acid. First heat the rosin, chalk, sand, and oil, in an iron boiler; then add the oxide, and with care, the acid: stir the composition carefully, and apply the coat while it is still hot. If it be not liquid enough, add a little more oil. This coating, when it is cold and dry, forms a varnish which is as hard as stone.—*Dingler's Polytech. Jour.—Bull. Soc. d'Encour. pour l'Indus. Nation.*

Growing of Plants by Electric Light.

At the meeting of the Academy of Sciences of Paris, of August 5th, 1861, M. Hervé Mangon presented the details of an experiment in which he had grown the seeds of rye under the influence of electric light alone. The plants assumed their green tint rapidly and vigorously, and showed no perceptible difference from those grown in ordinary day light.

Table of the Properties of Saturated Steam. Calculated by L. O.

From the Lond. Artizan, Sept., 1861.

In examining the existing tables of the properties of steam, we find that very few of them are correct, some because they are calculated from formulæ, based upon bygone and inaccurate experiments, and some because they are calculated from formulæ for steam in a gaseous state, which of course will not apply to saturated steam, generally used in practice. Having long felt the want for more correct tables for the properties of *saturated* steam, the Author has thought that the accompanying table, calculated by him, according to the newest and the best authorities upon steam, might be considered useful. In respect to the columns three and four, the Author is indebted to Mr. D. K. Clark for the use of his formulæ.

Atmosphere included.		Temperature of steam.	Specific volume.	Number of atmospheres.	Atmosphere excluded.	
Pounds per sq. inch.	Inches of mercury.				Inches of mercury.	Pounds per sq. inch.
lbs.	Inches.	Fahr.	Sp. vol.	Atmos.	Inches.	lbs.
1	2·0355	102·1	20582	·068	—27·886	—13·7
2	4·0710	126·3	10721	·136	—25·851	—12·7
3	6·1065	141·6	7322	·204	—23 815	—11·7
4	8·142	153·1	5583	·272	—21·780	—10·7
5	10·178	162·3	4527	·340	—19 744	— 9·7
6	12·213	170·2	3813	·408	—17·709	— 8·7
7	14·249	176·9	3298	·476	—15·673	— 7·7
8	16·284	182·9	2909	·544	—13·638	— 6·7
9	18·320	188·3	2604	·612	—11·602	— 5·7
10	20·355	193·3	2358	·680	— 9·567	— 4·7
11	22·391	197·8	2157	·748	— 7·531	— 3·7
12	24·426	202·0	1986	·816	— 5·496	— 2·7
13	26·462	205·9	1842	·884	— 3·460	— 1·7
14	28·497	209·6	1720	·952	— 1·425	— 0·7
14·706	29·922	212·0	1642	1·000	± 0·000	± 0·0
15	30·533	213·1	1610	1·020	0·611	0·3
16	32·568	216·3	1515	1·088	2·646	1·3
17	34·604	219·6	1431	1·156	4·682	2·3
18	36·639	222·4	1357	1·224	6·717	3·3
19	38·675	225·3	1290	1·292	8·753	4 3
20	40·710	228·0	1229	1·360	10·788	5·3
21	42·746	230·6	1174	1·428	12·824	6·3
22	44·781	233·1	1123	1·496	14·859	7·3
23	46·817	235 5	1075	1·564	16 895	8·3
24	48·852	237 8	1036	1·632	18·930	9·3
25	50·888	240·1	996	1 700	20·966	10·3
26	52·923	242·3	958	1·768	23·001	11·3
27	54·959	244·4	926	1·836	25·037	12·3
28	56·994	246·4	895	1·904	27·072	13·3
29	59·030	248·4	866	1·972	29·108	14·3
30	61·065	250·4	838	2·040	31 143	15·3
31	63·101	252·2	813	2·108	33·179	16·3
32	65·136	254·1	789	2·176	35·214	17·3

TABLE (CONTINUED).

Atmosphere included.		Temperature of steam.	Specific volume.	Number of atmospheres.	Atmosphere excluded.	
Pounds per sq. inch.	Inches of mercury.				Inches of mercury.	Pounds per sq. inch.
lbs.	Inches.	Fahr.	Sp. vol.	Atmos.	Inches.	lbs.
33	67·172	255·9	767	2·244	37 250	18·3
34	69·207	257·6	746	2·312	39·285	19·3
35	71·243	259·3	726	2·380	41·321	20·3
36	73·278	260·9	707	2·448	43·356	21·3
37	75·314	262·6	688	2 516	45 392	22·3
38	77·349	264·2	671	2·584	47·427	23·3
39	79 385	265·8	655	2·652	49·463	24·3
40	81·420	267·3	640	2·720	51·498	25·3
41	83·456	268·7	625	2·788	53·534	26 3
42	85·491	270·2	611	2·856	55·569	27 3
43	87·527	271·6	598	2·924	57·605	28·3
44	89 562	273·0	585	2·992	59·640	29·3
45	91·598	274·4	572	3·060	61·676	30·3
46	93 633	275 8	561	3 128	63·711	31·3
47	95·669	277·1	550	3·196	65·747	32·3
48	97·704	278·4	539	3 264	67·782	33·3
49	99·740	279·7	529	3·332	69·818	34·3
50	101·776	281·0	518	3·400	71·854	35·3
51	103·811	282·3	509	3·468	73·889	36·3
52	105·847	283·5	500	3 536	75·925	37·3
53	107·882	284·7	491	3·604	77·960	38·3
54	109 918	285·9	482	3·672	79·996	39·3
55	111·953	287·1	474	3·740	82 031	40·3
56	113·989	288·2	466	3·808	84 067	41·3
57	116·024	289·3	458	3 876	86·102	42·3
58	118·060	290·4	451	3·944	88·138	43·3
59	120·095	291·6	444	4·012	90·173	44·3
60	122 131	292·7	437	4·080	92·209	45 3
61	124·166	293·8	430	4·148	94·244	46·3
62	126·202	294·8	424	4·216	96·280	47·3
63	128·237	295·9	417	4·284	98·315	48·3
64	130 273	296·9	411	4·352	100·351	49 3
65	132·308	298 0	405	4·420	102·386	50·3
66	134·344	299·0	399	4·488	104·422	51·3
67	136·379	300·0	393	4·556	106·457	52·3
68	138·415	300·9	388	4·624	108·493	53 3
69	140·450	301·9	383	4·692	110·528	54·3
70	142·486	302·9	378	4·760	112 563	55·3
71	144·521	303 9	373	4·828	114·599	56·3
72	146·557	304·8	368	4·896	116·635	57·3
73	148·592	305·7	363	4·964	118·670	58·3
74	150 628	306·6	359	5·032	120·706	59 3
75	152·663	307·5	353	5·100	122·741	60 3
76	154·699	308·4	349	5·168	124·777	61·3
77	156·734	309·3	345	5·236	126 812	62·3
78	158·770	310·2	341	5·304	128 848	63·3
79	160 805	311·1	337	5·372	130 883	64·3
80	162·841	312·0	333	5·440	132 919	65·3
81	164·876	312·8	329	5·508	134·954	66·3

TABLE (CONTINUED).

Atmosphere included.		Temperature of steam.	Specific volume.	Number of atmospheres.	Atmosphere excluded.	
Pounds per sq. inch.	Inches of mercury.				Inches of mercury.	Pounds per sq. inch.
lbs.	Inches.	Fahr.	Sp. vol.	Atmos.	Inches.	lbs.
82	166 912	313·6	325	5·576	136 990	67·3
83	168·947	314·5	321	5·644	139·025	68·3
84	170·983	315·3	318	5·712	141·061	69·3
85	173·018	316·1	314	5·780	143 096	70·3
86	175·054	316·9	311	5·848	145·132	71·3
87	177·089	317·8	308	5·916	147·167	72·3
88	179·125	318·6	305	5·984	149·203	73·3
89	181·160	319·4	301	6·052	151·238	74·3
90	183·196	320·2	298	6·120	153·274	75·3
91	185·231	321·0	295	6·188	155·309	76·3
92	187·267	321·7	292	6·256	157·345	77·3
93	189·302	322·5	289	6·324	159·380	78·3
94	191·338	323·3	286	6·392	161·416	79·3
95	193·373	324·1	283	6·460	163·451	80·3
96	195·409	324·8	281	6·528	165·487	81·3
97	197·444	325·6	278	6·596	167·522	82·3
98	199·480	326·3	275	6·664	169·558	83·3
99	201·515	327·1	272	6·732	171·593	84·3
100	203·551	327·9	270	6·800	173·629	85·3
101	205 587	328·5	267	6 868	175·665	86·3
102	207·622	329·1	265	6·936	177·700	87·3
103	209·658	329·9	262	7·004	179·736	88·3
104	211·693	330·6	260	7 072	181·771	89·3
105	213·729	331·3	257	7·140	183 807	90·3
106	215·764	331·9	255	7·208	185 842	91·3
107	217·800	332·6	253	7·276	187 878	92·3
108	219 835	333·3	251	7·344	189·913	93·3
109	221·871	334·0	249	7·412	191·949	94·3
110	223·906	334·6	247	7·480	193·984	95·3
111	225·942	335·3	245	7·548	196·020	96·3
112	227·977	336·0	243	7 616	198·055	97·3
113	230·013	336·7	241	7·648	200·091	98·3
114	232·048	337 4	239	7·752	202·126	99·3
115	234·084	338·0	237	7 820	204·162	100·3
116	236·119	338 6	235	7·888	206·197	101·3
117	238·115	339·3	233	7·956	208·233	102·3
118	240·190	339 9	231	8·024	210·268	103·3
119	242·226	340 5	229	8·092	212 304	104·3
120	244·261	341·1	227	8·160	214·339	105·3
121	246·297	341·8	225	8·228	216·375	106·3
122	248·332	342·4	224	8·296	218·410	107·3
123	250·368	343·0	222	8·364	220 446	108·3
124	252·403	343·6	221	8·432	222·481	109·3
125	254·439	344·2	219	8·500	224·517	110·3
126	256·474	344·8	217	8·568	226·552	111·3
127	258·510	345·4	215	8 636	228 588	112·3
128	260·545	346·0	214	8·704	230·623	113·3
129	262 581	346·6	212	8·772	232·659	114·3
130	264·616	347·2	211	8 840	234·694	115·3

TABLE (CONCLUDED).

Atmosphere included.		Temperature of steam.	Specific volume.	Number of atmospheres.	Atmosphere excluded.	
Pounds per sq. inch.	Inches of mercury.				Inches of mercury.	Pounds per sq. inch.
lbs.	Inches.	Fahr.	Sp. vol.	Atmos.	Inches.	lbs.
131	266·652	347·8	209	8·908	236·730	116·3
132	268·687	348·3	208	8·976	238·765	117·3
133	270·723	348·9	206	9·044	240·800	118·3
134	272·758	349·5	205	9·112	242·836	119·3
135	274·794	350·1	203	9·180	244·872	120·3
136	276·829	350·6	202	9·248	246·907	121·3
137	278·865	351·2	200	9·316	248·942	122·3
138	280·900	351·8	199	9·384	250·978	123·3
139	282·936	352·4	198	9·452	253·014	124·3
140	284·971	352·9	197	9·520	255·049	125·3
141	287·007	353·5	195	9·588	257·085	126·3
142	289·042	354·0	194	9·656	259·120	127·3
143	291·789	354·5	193	9·724	261·156	128·3
144	293·113	355·0	192	9·792	263·191	129·3
145	295·149	355·6	190	9·860	265·227	130·3
146	297·184	356·1	189	9·928	267·262	131·3
147	299·220	356·7	188	9·996	269·298	132·3
148	301·255	357·2	187	10·064	271·333	133·3
149	303·291	357·8	186	10·132	273·369	134·3
150	305·327	358·3	184	10·200	275·405	135·3
155	315·504	361·0	179	10·540	285·582	140·3
160	325·682	363·4	174	10·880	295·760	145·3
165	335·859	366·0	169	11·220	305·937	150·3
170	346·037	368·2	164	11·560	316·115	155·3
175	356·214	370·8	159	11·900	326·292	160·3
180	366·392	372·9	155	12·240	336·470	165·3
185	376·569	375·3	151	12·580	346·647	170·3
190	386·747	377·5	148	12·920	356·825	175·3
195	396·924	379·7	144	13·260	367·002	180·3
200	407·102	381·7	141	13·600	377·180	185·3
210	427·457	386·0	135	14·280	397·535	195·3
220	447·812	389·9	129	14·960	417·890	205·3
230	468·167	393·8	123	15·640	438·245	215·3
240	488·522	397·5	119	16·320	458·600	225·3
250	508·878	401·1	114	17·000	478·956	235·3
260	529·233	404·5	110	17·680	499·311	245·3
270	549·587	407·9	106	18·360	519·666	255·3
280	569·943	411·2	102	19·040	540·021	265·3
290	590·297	414·4	99	19·720	560·376	275·3
300	610·653	417·5	96	20·400	580·731	285·3
350	712·429	430·1	83	23·800	682·507	335·3
400	814·204	444·9	73	27·200	784·282	385·3
450	915·980	456·7	66	30·600	886·058	435·3
500	1017·755	467·5	59	34·000	987·833	485·3
600	1221·306	487·0	50	40·800	1191·384	585·3
700	1424·857	504·1	43	47·600	1394·935	685·3
800	1628·408	519·5	38	54·400	1598·486	785·3
900	1831·959	533·6	34	61·200	1802·037	885·3
1000	2035·510	546·5	31	68·000	2005·588	985·3

The Boiling-points of Different Liquids.

From the London Chemical News, No. 83.

The laws relating to the boiling-points of different liquids at the ordinary pressure of the atmosphere have lately been investigated by Mr. Tate, and the results of his experiments are published in the *Philosophical Magazine*. He has made experiments with solutions containing the chlorides of sodium, potassium, barium, calcium, and strontium; the nitrates of soda, potassa, lime, and ammonia; and the carbonates of soda and potassa. He has found for all these salts that the augmentation of boiling temperature may be approximately expressed in a certain power of the per centage of the salt dissolved. The salts enumerated may be divided into four distinct groups; namely, first, the chlorides of sodium, potassium, and barium, and the carbonate of soda; second, the chlorides of calcium and strontium; third, the nitrates of soda, potassa, and ammonia; fourth, the carbonates of potassa and nitrate of lime. In each of these four groups, the augmentations of boiling temperature of the solutions have a constant ratio to one another for an equal weight of salt dissolved. He has also ascertained by experiments that for an equal weight of salts, the boiling temperatures are (approximately) in the inverse ratio of the chemical equivalents of their bases, and in the case of the nitrate of lime and the carbonate of potassa with the equivalents of the entire salts. Although the law thus indicated is not strictly true, it is sufficiently exact to warrant further inquiry, and the cases in which it is found to apply are too numerous to be referred to accidental coincidence. Future researches may extend these laws to other substances, as it is quite consistent with analogy to suppose that the chemical composition of a substance affects the boiling temperature of its solution. It will readily be acknowledged that the prosecution of these experiments may throw additional light upon the generation of steam, the economy of fuel, and kindred questions of great practical importance to engineers.

For the Journal of the Franklin Institute.

On the Economy of Using Steam Expansively. By H. P. M. Birkinbine, Chief Engineer of the Philadelphia Water Works.

The economy of using steam expansively has been much before the public of late, and many conflicting statements have been published, evidently produced by the Report of the Erie Experiments made by the United States Naval Engineers, in which the conclusions were contrary to the usually admitted experience of engineers.

A rough experiment made with one of the pumping engines of the Water Works of this city, may not be uninteresting. This engine was originally so arranged as to carry steam the entire length of the stroke; the valve gear was much worn, and defective in its arrangement; a new valve gear was put upon the engine, by which the steam could be cut off at any point of the stroke. The experiments to test

the value of this improvement were not made with the precision and care they should have been for publication, but were made simply for the use and information of the department. The results may, however, be taken as practically correct, although more carefully conducted experiments might make a slight difference.

The experiments were commenced immediately after cleaning the fires and coaling, and terminated when the fires required the final cleaning and coaling; the time being 28 and 28½ hours, with but a slight interval between the experiments, simply to alter the position of the cams which opened the valves. In firing, the usual practice of the department was observed, viz: keeping a clean fire, coaling frequently, clinkering and thoroughly cleaning the fires about once in twelve hours. The coal used was anthracite (Lehigh white ash) of fair quality. The load upon the engine and the condition of the engine pump and boilers were the same in both experiments. In the first experiments, and with the old valve gear, it required careful firing to keep the engine in motion at any thing like a fair speed, say twelve revolutions per minute. In the experiment with steam cut off at half stroke, as the engine is now running, it is kept up with ease. Steam was not cut off at less than half stroke, on account of the boilers, which are old, and not considered perfectly safe at over 60 pounds pressure. It will be seen that this engine is not economical in fuel, nor is it constructed on the most approved plan, but these experiments show the economy of using steam expansively, in this instance at least.

Description of Engine.—The engine upon which the experiments were made is a non-condensing one, included within the operations of the Philadelphia Water Department, and forms part of the machinery for supplying the inhabitants of what was formerly the District of Kensington with water. It has a horizontal steam cylinder, 30 inches in diameter, and works a double-acting pump, 18 inches in diameter, each piston having a stroke of six feet. The pump is placed horizontally about 25 feet in front of the steam cylinder, and in a plane 18 feet below it. The piston-rod of the cylinder gives motion to the upper end of the vibrating beam, by means of a cross-head and a short connecting rod, while a similar cross-head and rod transmit the motion of the lower end of the beam to the piston-rod of the pump. From the upper end of the beam a connecting-rod also gives motion to a crank-shaft and fly-wheel, by means of which the motion of the engine is equalized. The pump receives its water from the river under a head of about four feet at mean tide, and forces it through an 18-inch pipe 13,260 feet long to a reservoir 118 feet above the average level of the river.

The steam and exhaust valves of the cylinder are of the "Cornish equilibrium" variety, placed in chests at each end of the cylinder, and are operated by cams on a revolving shaft driven by bevel-wheels from the crank-shaft of the engine.

The arrangement for varying the "cut-off" is simple and effective. Each steam valve is opened by a roller on the end of an appropriate

lever, which is depressed by a raised face or projection on a corresponding cam. The cams are so made that by moving them longitudinally on the shaft, faces of greater or less length, corresponding to different grades of expansion, are presented to the roller on the end of the valve lever.

Steam is generated in six cylindrical boilers, set in brick-work. Under each boiler, in the chamber behind the bridge-walls, is a supplementary boiler or heater, connected to the main boiler by wrought iron pipes.

PRINCIPAL DIMENSIONS OF ENGINE AND BOILERS.

Diameter of cylinder of engine,	30 ins.
Length of stroke of piston of engine,	6 ft.
Cubical contents of nozzle and clearance at each end of cylinder,	1·5 cub. ft.
Diameter of pump cylinder,	18 ins.
Stroke of piston of do.	6 ft.
Diameter of fly-wheel,	22 ft.
Weight of rim of do.,	13,000 lbs.

Boilers.—Six cylindrical boilers set in brick-work. To the under side of each boiler is connected a cylindrical heater or drum.

Diameter of Boilers,	40 ins.
Length of do.,	26 ft.
Diameter of heaters,	30 ins.
Length of do.,	16½ ft.
Total heating surface, including one-half of the whole surface of boilers and the whole of the surface of heaters, about	1500 sq. ft.
Area of grate surface,	200 "

OBSERVED RESULTS OF EXPERIMENTS.	Without Expansion.	With Expansion.
Duration of experiments,	28·5	28
Total number of revolutions,	21,860	26,250
Total pounds of coal consumed,	21,520	16,270
Pressure of steam as per gauge on boilers,	38	50
Average effective pressure per square inch of steam piston as per indicator diagram,	28·5	28·5
DEDUCTIONS FROM OBSERVED RESULTS.		
Revolutions per minute,	12·78	15·62
Revolutions per pound of coal,	1·015	1·613
Coal per horse power per hour,	8·073	5·083
Coal consumed per hour per square foot of grate,	3·75	2·9
Coal consumed per revolution, in pounds,	·984	·619
Water evaporated per pound of coal, in pounds,	6·72	5 85
Coal saving with expansion in per centums of coal used without expansion,		37·03

For the Journal of the Franklin Institute.

Strength of Cast Iron and Timber Pillars: A series of Tables showing the Breaking Weight of Cast Iron, Dantzic Oak, and Red Deal Pillars. By Wm. Bryson, Civ. Eng.

(Continued from vol. xlii., page 405.)

In the *Builder*, vol. v, page, 18,—Jan. 9th, 1847,—the following "Question as to the Strength of Cast Iron," was published:—

"Suppose I wish to settle the scantlings of some iron castings, such as girders, pillars, &c., and suppose that, with the intention of resting my case upon the best and soundest authority, I consult Tredgold's well-known Essay on Cast Iron, in which I find some beautiful theories of the action of forces upon materials, joined to what the author seems to have considered sufficiently sound data for the strength of the particular material, cast iron. But, suppose my copy of the book is one of the fourth edition (edited by Mr. E. Hodgkinson, 1842), in which I find in the editor's notes some slur cast upon Tredgold's accuracy, principally as regards the varying strength of iron of different manufactures, of which many sorts are said to be very much below the author's assumed average:—

"Now, I wish to ascertain whether, if my girders, &c., are procured from manufacturers of established credit and respectability, in London or elsewhere, they may not be calculated to possess the degree of strength, at elastic limits, which is assumed in Tredgold's formulæ? If not, what may be expected to happen to the numerous buildings in which cast iron supports have been used, but which have been erected previously to the recent researches of Mr. Hodgkinson and Mr. Fairbairn? I have not yet procured a detailed copy of these: when I do, shall I have to unlearn Tredgold's beautiful system of calculating pillars and struts, or shall I only be obliged to modify his constants to suit another scale of strength?"

Weisbach says, "In 1817, Barlow's Essay on the 'Strength of Timber, Iron, and other materials,' was published, and English engineers were thus put far on the way of making 'principles of science rules of their art.' A few years afterwards, Tredgold's Essay 'On the Strength of Cast Iron and other Metals,' was published; and this remarkable work of a most remarkable man, together with Barlow's work, had, all engineers will admit, a powerful influence in extending the rational use of iron in construction. Ten years later, Mr. Eaton Hodgkinson of Manchester, began a course of inquiry on the strength of iron, which, while it has earned for him and his coadjutor, Mr. Fairbairn, a high reputation for scientific knowledge and skill, has, even more directly than the earlier works mentioned, contributed to the present important position of iron as a material in construction."

Mr. Fairbairn in his report on the Construction of Fire-proof Warehouses, says that Mr. Hodgkinson is "one of the first authorities in this or any other country on the strength of materials. To that gen-

tleman the public are indebted for a series of theoretical and practical experiments on the strength of beams and pillars, of the utmost value to architects, builders, and engineers. Any person choosing to make himself acquainted with the principles of Mr. Hodgkinson's experiments and the results deduced therefrom, will find no difficulty in constructing beams and columns of the strongest form, and at the same time insuring the proportional and requisite strength, accompanied with a great saving in material in all parts of the structure."

Mr. Hodgkinson in his "Experimental Researches," remarks that "The acknowledged want of practical information upon this subject (the strength of pillars), and its great importance, made me anxious to undertake an extensive series of experiments upon it, such as would confirm or show the error of existing theories, and give such information as would be of real service to the engineer and architect, whilst they tended to unfold the laws that regulate the strength of pillars. This wish was, as on other occasions, cheerfully responded to by my friend William Fairbairn, Esq., at whose expense the extensive series of experiments was made."

Extracts from Mr. Hodgkinson's Experimental Researches:—

"In all long pillars of the same dimensions, the resistance to fracture by flexure is about three times greater when the ends of the pillar are flat and firmly bedded, than when they are rounded and capable of turning."

"The strength of a pillar, with one end round and the other flat, is the arithmetical mean between that of a pillar of the same dimensions with both ends rounded, and with both ends flat. Thus, of three cylindrical pillars, all of the same length and diameter, the first having its ends rounded, the second with one end rounded and one flat, and the third with both ends flat, the strengths are as 1, 2, 3, nearly."

"A long uniform pillar, with its ends firmly fixed, whether by discs or otherwise, has the same power to resist breaking as a pillar of the same diameter, and half the length, with the ends rounded or turned so that the force would pass through the axis."

"The preceding properties were found to exist in long pillars of steel, wrought iron, and wood."

"A pillar irregularly fixed, so that the pressure would be in the direction of the diagonal, is reduced to one-third of its strength, the case being nearly similar to that of a pillar with rounded ends, the strength of which has been shown to be only one-third of that of a pillar with flat ends."

NOTE.—"Tredgold, art. 283 of his work on Cast Iron, and in his Treatise on Carpentry, following the idea of Serlio in his Architecture, recommends circular abutting joints to lessen the effect of irregularity in the strains upon columns, from settlements and other causes; but this, we see, is voluntarily throwing away two-thirds of the full strength of the material to prevent what may often be avoided."

Mr. Tredgold remarks, "The writer of the article 'Bridge,' in the Supplement to the *Encycl. Brit.*, has shown that, when the force acts in the direction of the diagonal of the block (or column), the strain will be twice as great as when the same force acts in the direction of the axis. Now, the reader will be satisfied that, in consequence of settlements or other causes, a column is always liable to be strained in this manner, and therefore will carefully avoid enlarging the ends of his columns under the notion of gaining stability, for the effect of the straining force will be still more increased by such enlargement in the event of a change of direction from settlement. In my 'Treatise on Carpentry,'" Mr. T. says, "I have recommended circular abutting joints to lessen the effect of a partial change in the position of the strained pieces: an idea which appears to have occurred, in the first instance, to Serlio."

Mr. Fairbairn, in treating "On the Construction of Fire-proof Warehouses," says, "The base of the lower column should in every case be considerably enlarged, and the ends faced in the lathe; the base-plate which receives it should also be faced. This is the more necessary, as it gives an even surface for the purpose of leveling the plate and maintaining the vertical position of the column. The same operation is performed on the upper end of the socket, and on the bottom of each succeeding column."

In Tredgold's Essay on the Strength of Cast Iron, the following formula is given for the strength of a solid cylindrical column of cast iron to resist compression in the direction of its length, when the force acts in the direction of one of the surfaces of the column; d representing the diameter in inches, l the length in feet, and w the weight to be supported in lbs.,

$$\frac{9562\,d^4}{4\,d^2 + \cdot 18\,l^2} = \text{w}.$$

And in this *Journal*, vol. xli, page 248, Mr. Haswell gives a similar formula for the safe weight of a solid cylindrical column of cast iron, thus,

$$\frac{10{,}000\,d^4}{4\,d^2 + \cdot 18\,l^2} = \text{w}.$$

Mr. Tredgold remarks of his table calculated from the above formula, that it "shows by inspection the weight or pressure a cylindrical pillar or column of cast iron will bear with safety. The pressure is expressed in cwts., and is computed on the supposition that the pillar is under the most unfavorable circumstances for resisting the stress, which happens when, from settlements, imperfect fitting, or other causes, the direction of the stress is in the surface of the pillar." An abstract of this table, and also of Mr. Haswell's, is given further on.

Mr. Tredgold gives the following example of the use of his table.

"If it be desired to fix on the diameter for story posts of cast iron to support the front of a house; such a one, for example, as is commonly erected in London, where the ground story is to be occupied with shops. In such a case, each foot in length of frontage may be estimated at 25 cwts. for each floor, and 12 cwts. for the roof; hence in a house with three stories over the shops, the extreme load will be

$$(3 \times 25) + 12 = 87 \text{ cwts.},$$

on each foot of frontage. Now, if the posts be 7 feet apart, and 12 feet high, we have $7 \times 87 = 609$ cwts., the load upon one post; and hence we find by the table, that a pillar $6\frac{1}{2}$ inches in diameter would be sufficient; the load 525 cwts., which corresponds to a diameter of 6 inches, being too small.

"If there be only two stories above the pillars, and the height of a pillar be 10 feet, the distance from pillar to pillar 7 feet; then

$$\overline{(2 \times 25) + 12} \times 7 = 434 \text{ cwts.},$$

the whole load for one pillar; and it appears by the table, that a pillar 5 inches in diameter would sustain 452 cwts.; consequently, 5 inches will be a proper diameter for the pillars.

"When pillars are placed at irregular distances, that which carries the greatest load should be calculated for; and if it happen that such a pillar stands 10 feet from the next support on one side, and 6 feet from the next support on the other side, add these distances together, and take the mean for the distance apart; thus,

$$\frac{10 + 6}{2} = \frac{16}{2} = 8,$$

the mean distance of the supports.

"The strain upon a pillar cannot be exactly in the direction of the axis when the pillars are placed at unequal distances to support an uniform load; and since this unequal distribution of supports is extremely common in story posts, the propriety of adopting the mode of calculation I have followed is evident."

Abstract from Mr. Tredgold's table, entitled

"*A Table to show the Weight or Pressure a Cylindrical Pillar or Column of Cast Iron will sustain with Safety, in Hundredweights.*"

Diameter in inches.	Length or height in Feet.							
	6	8	10	12	14	16	18	20
	Weight in cwts.	Weight in cwts.	Weight in cwts.	Weight in cwts.	Weight in cwts.	Weight in cwts.	Weight in cwts.	Weight in cwts.
5	501	479	452	427	394	365	337	310
6	592	573	550	525	497	469	440	413
7	1013	989	959	924	887	848	808	765
8	1315	1289	1259	1224	1185	1142	1097	1052
9	1697	1672	1640	1603	1561	1515	1467	1416

Table showing the calculated weight from Mr. Tredgold's formula, in hundredweights and tons,

$$\frac{9562\,d^4}{4\,d^2 + \cdot 18\,l^2} = \text{W}.$$

Diameter in inches.	Length or height in Feet.							
	6	8	10	12	14	16	18	20
	cwts.	cwts.	cwts.	cwts.	cwts.	cwts.	cwts.	cwts.
5	504·11	478·47	452·19	423·75	394·42	365·26	337 02	310·22
6	735·28	711·45	683·00	651·16	617·16	582·09	546·88	512·25
7	1012·36	987·78	957·87	923 68	886 30	846 75	806·00	764·86
8	1332·26	1307·16	1276·25	1240·40	1200 54	1157·62	1112·54	1066·14
9	1694·93	1669·48	1637·84	1600·77	1559·07	1513·57	1465 11	1414·51
	tons.	tons.	tons.	tons.	tons.	tons.	tons.	tons.
5	25·05	23·92	22 60	21·18	19·72	18·26	16·85	15·51
6	36·76	35·57	34·15	32·55	30·85	29·10	27·34	25 61
7	50·61	49·38	47 89	46·18	44·31	42 33	40·30	38 24
8	66·61	65·35	63·81	62 02	60·02	57 88	55·62	53·20
9	84·74	83 47	81·89	80·03	77 95	75 67	73·25	70·72

Abstract from Mr. Haswell's table, entitled

"*Table showing the Weight or Pressure a Column of Cast Iron will Sustain with Safety.*"

	Length or height in Feet.							
	6	8	10	12	14	16	18	20
Inch.								
5	58,617	56,043	52,881	49,959	46,098	42,705	39,439	36,270
6	69,264	67,041	64,350	61,425	58,149	54,873	51,480	48,321
7	118,521	115,713	112,203	108,108	103,779	99,216	94,536	89,505
8	153,855	150,813	147,303	143,208	138,645	133,614	128,349	123,084
9	198,549	195,624	191,880	187,551	182,637	177,255	171,639	165,672

Table showing the calculated weight from Mr. Haswell's formula, in pounds and tons,

$$\frac{10{,}000\,d^4}{4\,d^2 + \cdot 18\,l^2} = \text{W}.$$

Diameter in inches.	Length or height in Feet.							
	6	8	10	12	14	16	18	20
	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.
5	58,696	56,043	52,966	49,634	46,200	42,784	39,477	36,337
6	86,124	83,333	80,000	76.271	72,289	68,181	64,056	60,000
7	118,579	115,699	112,196	108,192	103,813	99.182	94,408	89,589
8	156,049	153,110	149,489	145,289	140,620	135,593	130,313	124,878
9	198,529	195,547	191,842	187,500	182,615	177,285	171,610	165,681
	tons.	tons.	tons.	tons.	tons.	tons.	tons.	tons.
5	26·20	25·01	23·64	22·15	20·62	19·10	17·62	16·22
6	38·44	37·20	35 71	34 04	32·27	30 43	28 59	26·78
7	52·93	51·65	50·08	48·30	46·34	44·27	42·14	39·99
8	69·66	68·35	66·73	64·86	62·77	60 53	58·17	55·74
9	88·62	87·29	85 64	83·69	81·52	79·14	76·61	73·96

Tables from my calculations, being one-tenth and one-fourth of the breaking weight as deduced from Mr. Hodgkinson's formulæ for solid pillars of cast iron with both ends flat and firmly fixed.

TABLE SHOWING ONE-TENTH OF THE BREAKING WEIGHT, IN TONS.

Diameter in inches.	Length or height in Feet.							
	6	8	10	12	14	16	18	20
	tons.	tons.	tons.	tons.	tons.	tons.	tons.	tons.
5	45·07	33 75	25·97	19·57	15·06	12·00	9·82	8·21
6	74 67	57·85	45·60	36·66	28·77	22·93	18·77	15·69
7	112·68	89·87	72·39	63·91	49·06	39·63	32·44	27·12
8	159·15	130·11	106·86	88·63	70 59	63·13	52 12	43·57
9	214·07	178·75	149·36	125·59	106·53	91·23	78·86	66·19

TABLE SHOWING ONE-FOURTH OF THE BREAKING WEIGHT, IN TONS.

5	112·68	84·39	64·94	48·94	37·66	30·01	24·56	20·53
6	186·69	144·64	114·01	91·66	71·94	57·33	46·92	39·23
7	281·71	224·68	180·98	159·79	122·65	99·09	81·11	67·81
8	397·89	325·29	267·16	221·58	176·47	157·84	130·30	108·93
9	535·19	446·88	373·40	313·98	266·34	228·08	197·16	165·49

Table showing One-Fourth of the Calculated Breaking Weight, in Tons,

As deduced from Mr. Hodgkinson's formulæ for cast iron pillars with rounded ends, as will be seen by referring to a table comparing the strength of pillars, further on.

Diameter in inches.	Length or height in Feet.							
	6	8	10	12	14	16	18	20
	tons.	tons.	tons.	tons.	tons.	tons.	tons.	tons.
5	70·10	46 13	31·56	23·15	17·81	14·19	11·62	9·71
6	126·42	90·26	62·65	45·95	35·36	28·18	23·05	19·28
7	202·65	149·04	111·86	82·05	63·13	50·31	41·18	34·42
8	300·58	227·22	175·99	135·56	104·31	83·12	68·04	56 88
9	420·74	326·09	257 11	200·77	162·42	129·44	105·95	88·57

Solid Uniform Cylindrical Pillars of Cast Iron, Both Ends being Flat and Firmly Fixed.

Length or height of Pillar in feet.	Number of diameters contained in the length or height.	Diameter in inches.	Calculated weight of metal contained in pillar in lbs.	Calculated breaking weight in tons from formula, $w = 44{\cdot}16\frac{D^{3{\cdot}55}}{L^{1{\cdot}7}}$.	Calculated breaking weight in tons from formulæ, $w = 44{\cdot}16\frac{D^{3{\cdot}55}}{L^{1{\cdot}7}}$. $Y = \frac{w\,c}{w + \frac{2}{3}c}$.
6	10 2-7	7	722·37		1126·87
8	13 5-7	"	963·16		898·74
10	17 1-7	"	1203·95		723 93
12	20 4-7	"	1444·74		639·16
14	24	"	1685·53		490·63
16	27 3-7	"	1926 32	396·38	
18	30 6-7	"	2167·11	324·46	
20	34 2-7	"	2407·90	271·24	
6	9	8	943·50		1591·59
8	12	"	1258 00		1301·19
10	15	"	1572·50		1068·65
12	18	"	1887·01		886·34
14	21	"	2201 51		705·91
16	24	"	2516·01		631·38
18	27	"	2830·51	521 23	
20	30	"	3145·01	435·74	
6	8	9	1194·12		2140·76
8	10 2-3	"	1592·16		1787·55
10	13 1-3	"	1990·20		1493·63
12	16	"	2388·24		1255·95
14	18 2-3	"	2786·28		1065·36
16	21 1-3	"	3184·32		912·33
18	24	"	3582·36		788·67
20	26 2-3	"	3980·40	661·96	

Solid Cylindrical Pillars of Dantzic Oak, Both Ends being Flat and Firmly Fixed.

Length or height of Pillar in feet.	Number of diameters contained in the length or height.	Diameter in inches.	Cubical content in feet.	Approximate weight of pillar in lbs.	Calculated breaking weight in tons from formula, $w = 6{\cdot}71\frac{D^4}{L^2}$.	Value of w.	Value of c.	Calculated breaking weight in tons from formula, $x = \frac{wc}{w+\frac{1}{2}c}$.
8	13 5-7	7	2·138	100·86		251·72	132·80	95·15
9	15 3-7	"	2·405	113·46		198·89	"	88·48
10	17 1-7	"	2 672	126·07		161·10	"	82 06
11	18 6-7	"	2 939	138·68		133·14	"	75·96
12	20 4-7	"	3·207	151·29		111·87	"	70·25
13	22 2-7	"	3·474	163·89		95·32	"	64·94
14	24	"	3·741	176 50		82·19	"	60·04
15	25 5-7	"	4·008	189·10		71·60	"	55·54
16	27 3-7	"	4·276	201·72		62 93	"	51·41
17	29 1-7	"	4·543	214·32		55·74	"	47·65
18	30 6-7	"	4 810	226 93		49·72	"	44·21
19	32 4-7	"	5·077	239·54		44·62	"	41·08
20	34 2-7	"	5·345	252·15		40·27	"	38·23
21	36	"	5·612	264·75		36·53	"	35·63
22	37 6-7	"	5·879	277·36		33·28	"	33·25
23	39 3-7	"	6·146	289·97	30·45			
24	41 1-7	"	6·414	302·58	27·96			
25	42 6-7	"	6·681	315·18	25·77			
26	44 4-7	"	6·948	327·79	23·83			
27	46 2-7	"	7·215	340·40	22·09			
28	48	"	7·483	353 01	20·54			
29	49 5-7	"	7·750	365·61	19·15			
30	51 3-7	"	8 017	378 22	17·90			

Solid Cylindrical Pillars of Red Deal, Both Ends being Flat and Firmly Fixed.

					$w = 4{\cdot}79\frac{D^4}{L^2}$.			
8	13 5-7	7	2·138	93·10		179·69	113·14	76·85
9	15 3-7	"	2·405	104·73		141·98	"	70 81
10	17 1-7	"	2·672	116·37		115·00	"	65·10
11	18 6-7	"	2·939	128·01		95 04	"	59·77
12	20 4-7	"	3·207	139·65		79·86	"	54 85
13	22 2-7	"	3·474	151·29		68·05	"	50·35
14	24	"	3·741	162·92		58·67	"	46·11
15	25 5-7	"	4·008	174·56		51·11	"	42·53
16	27 3-7	"	4·276	186·20		44 92	"	39·16
17	29 1-7	"	4·543	197·84		39·79	"	36·11
18	30 6-7	"	4·810	209·47		35·49	"	33·36
19	32 4-7	"	5·077	221·11		31·85	"	30·87
20	34 2-7	"	5·345	232·75		28·75	"	28·63
21	36	"	5·612	244·39	26 07			
22	37 6-7	"	5·879	256·02	23·76			
23	39 3-7	"	6·146	267·66	21·74			
24	41 1-7	"	6·414	279 30	19·96			
25	42 6-7	"	6 681	290 94	18·40			
26	44 4-7	"	6·948	302·58	17·01			
27	46 2-7	"	7·215	314·21	15·77			
28	48	"	7·483	325·85	14·66			
29	49 5-7	"	7·750	337·49	13·67			
30	51 3-7	"	8·017	349·13	12·77			

(To be Continued.)

PROCEEDINGS OF THE BRITISH ASSOCIATION.

From the London Athenæum, Sept., 1861.

SECTION G.—*Mechanical Science.*

Mr. D. Chadwick, Secretary of the Manchester Cotton Supply Association, read a paper, "ON RECENT IMPROVEMENTS IN COTTON GINS." A description was given of the old Indian churka, one of which was exhibited to the meeting, and the invention of the American saw gin, by Eli Whitney, was also noticed and described. On the recent visit of Dr. Forbes, the superintendent of the cotton gin factory of the late East India Company, to Darwhar, he introduced an improved cotton gin, based upon the principle of the Indian churka. This churka gin had subsequently been improved by Mr. John Dunlop, of Manchester, and Messrs. Platt Brothers, of Oldham, and the improved machines were exhibited to the meeting. The improvements in Messrs. Platt's machines consisted in the application of spike rollers revolving at different speeds in connexion with vibrating machinery, which transmits the cotton to the ordinary churka rollers. The effect of this is to enable the machine to be supplied with cotton at intervals instead of continuously with the fingers. The machine is intended to be worked by power, and requires the attendance only of a child thirteen years of age. Mr. Dunlop's machine was less expensive, more compact, bearing a closer resemblance to the original churka, and was intended to be worked by hand.

Mr. T. Bazley, M. P., said the machines before them were wonderful improvements on the old churka. He noticed the destruction of fibre and the waste occasioned by the American saw gins, and said he had seen cotton in the market selling for 7d. per lb., which, if cleaned by a roller gin, would have sold for 2s. per lb. The injuries inflicted upon the raw cotton were not so great as upon the long fibre cotton, because the teeth of the saws allowed the short fibres to go through without severing them. During the last few years, an improved kind of roller gin, known as the Macarthey Gin, had been introduced into America. An intimate friend of his had obtained one of these gins, and placed it in the hands of Mr. Dunlop, who had made a large number of these gins, which the Cotton Supply Association had forwarded to the various cotton-producing districts of the world. But when he turned to the machine which had been constructed by Mr. Platt, that appeared so him to be the machine best adapted for the cleaning of a very large quantity of cotton in a short time without injury to the fibre. He was very glad to see these machines in the room, though he feared they would soon be in the position of the cook who had all the appliances for cooking a good dinner, but was without the mutton and the beef to cook. He was afraid, unless very serious efforts were made, this great industry of theirs would be very much depressed.

Mr. Ashworth said he believed the Indian cotton, which, as now cleaned, was worth 4d. per lb., would be worth 5d. per lb., if cleaned by the cotton gins exhibited.

Mr. J. F. Bateman, President of the Section, made a communication, "On STREET PIPE ARRANGEMENTS FOR EXTINGUISHING FIRES." He had hoped that a paper would have been read on this subject by Mr. Rose, of the Manchester Fire Brigade, but as that gentleman had been called away by the illness of a relative, he (Mr. Bateman) thought it right that the proceedings of the Section should not terminate without some observations being made on the subject. Nothing could have been much worse than the arrangements made for the extinction of fires some fifteen years ago, and nothing could be much worse than the state of things which existed at the present day in the City of London. In most large towns, as Manchester and Glasgow, for instance, where the supply of water had been taken into the hands of the Corporation, the best preparations had been made for the extinction of fires. But in London, the fire engines and the brigade were maintained by contributions from the different insurance companies, and it was therefore evident that their interest only lay in preventing the destruction of property that was insured. It was clear this was a state of things which ought not to exist in this country. Some twelve or fifteen years ago he turned his attention to the subject of the extinction of fires. The old wooden plug or fire-cock was then generally in use, and it still continued in use in some parts of the country. Mr. Bateman described the construction of the branch stand-pipe, with which he had replaced the old plugs in Manchester and other towns, and stated that as a general rule these stand-pipes had been found sufficient without the use of fire engines. He also explained the principle upon which the water-pipes were laid down in Manchester; so that within reach of nearly every block of valuable buildings in Manchester and the neighborhood, there were from two to three sources of water supply, and ten or twelve fire-cocks within a hundred yards. Then came the question of pressure. It was popularly supposed that water could be thrown to any height; but this was not so. About eighty or ninety feet was the greatest height water could be thrown by a fire engine. The highest mills in Manchester were from forty feet to sixty feet, and experiments had been made to show that at the low pressure the stand-pipes would throw ninety feet.

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Mr. C. W. Siemens explained a system of telegraphic communication adopted in Berlin in the case of fires, by means of which, immediately after a fire occurred, the police at every station in the town could be informed of the occurrence, and of the district in which the fire had occurred. He said it was found by the adoption of this system that the fire engine was generally on the ground five minutes after the alarm had been given. He also explained and exhibited a system of railway signalling extensively adopted on the Continent, which rendered collisions almost impossible.

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Col. Sir H. James, R. E., described the process of "PHOTOZINCOGRAPHY," by means of which photographic copies of the Ordnance maps are cheaply multiplied, either on their original or on a reduced

or enlarged scale. The process is applicable to the reproduction of old manuscripts and old printed books. A copy of Domesday Book (the part relating to Cornwall taken by this means) was exhibited to the meeting. The process consists in taking a photographic collodion negative, which is intensified by means of bichloride of mercury and sulphate of ammonia. Paper deprived of its size is saturated with a solution of gelatine and bichromate of potash. The paper thus prepared is exposed to the light beneath the negative, the result of which is that the parts which have been exposed to the light become hardened and insoluble. The whole is then inked with a greasy ink, and afterwards washed in water, which removes the ink from all the parts except those on which the light has acted. A transfer to stone or zinc is then taken in the ordinary way, and copies are printed. Sir Henry James then described an improvement which had lately been made in the process, by means of which a reduced copy of a map or plan could be made, in which the minor detail (which would be useless on a reduced scale) could be omitted, and the names of places and other features of the plan given in full-sized legible characters.

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Mr. Haworth read a paper explaining his patent for IMPROVEMENTS IN STREET RAILWAYS, by the addition of a fifth or perambulator wheel to the carriages, running as a guide in a central groove between the trams. It was calculated that a saving of 35 per cent. would be effected by this plan.

Mr. Vignoles expressed his opinion that if any street railway were ever adopted, Mr. Haworth's system would be the one. He had never seen a more promising system.

Appalling Boiler Catastrophe.

From the Lond. Artizan, Oct. 1861.

The vicinity of Rotherhithe, near the Commercial Docks, was, on the evening of the 16th ult., the scene of a most frightful catastrophe, occasioned by the explosion of a steam boiler at the Lower Ordnance Wharf, in the occupation of Messrs. Francois and Joseph Badart, oil cake manufacturers, by which no fewer than ten persons lost their lives. The premises occupy a large area on that portion of the water-side which is known as Cuckold's Point, opposite to the Limehouse entrance of the West India Docks, and about half a mile from the Commercial Dock-pier. They comprised several buildings, wherein the processes of grinding the seed, extracting and refining the oil, and compressing the oil cakes, were carried on. The machinery, which was very extensive, was propelled by steam power. The boiler and engine-house stood on the river side of the mills, parallel with the wharf, and contained two long boilers, of 50 horse-power each, laid in massive brick-work. The number of hands generally employed in the mills is somewhat limited, considering their extensive character, but owing to the pressure of business and the urgency of some shipping orders, a relay of workmen was taken on on Monday evening at

six o'clock, for night work. Only one boiler had been at work during the day; and it appears that about the same time the night men came on, six o'clock, a defect was noticed in a joint of the feed pipe to the engine. The engine-driver at once sent for an engine-fitter, at the same time turning the steam off the boiler. It would seem, however, that the driver, acting under the impression, probably, that the joint would speedily be repaired, had omitted to draw the furnace, as a quantity of fire is stated to have been in it. The fitter arrived, and, with the driver, proceeded down to the engine-room and commenced their work, but finding that they required assistance, they communicated with one of the officials, and the whole of the laborers who had just come on were sent to their aid. Six of the men were desired to support the pipe which runs across the roof, while the engine-fitter went on with repairing the joint. There had been three or four other laborers in the chamber, but, their services not being immediately required, they had only left a few minutes before the explosion took place. The men had not been long at work at the joint before those in other parts of the factory were alarmed by a loud rumbling noise and heavy concussion, which shook the neighborhood, followed by a terrific crash, and a rush of steam and smoke. The boiler-house was seen to be in ruins, and at the time it was difficult to say what would be the fate of the whole property. It was some minutes ere any attempt could be made to approach the engine-room, where the unhappy men had been at work. At length, the smoke and steam having somewhat subsided, two of the men who had only left the place a minute or so before, and two or three other laborers, contrived to make a descent to where the poor creatures were imprisoned, and the sight which presented itself was truly horrible. The engine-room was a small brick chamber, about 8 ft. by 12 ft., at the basement of the mills. A glance at the boiler clearly showed that some portion of it had exploded, and had sprung forward from its bed of brick-work four or five feet, and that the inner part of the boiler, with the massive iron bars of the furnace and other plates, had been blown out, as if from a cannon's mouth, direct at the poor fellows who were at work only some four or five feet in front. Of the ten unhappy creatures who were in this chamber, not one escaped. Two or three were dashed against the brick wall and killed on the spot, their skulls being driven in by the iron bars and pieces of boiler which were scattered by the explosion. Others were frightfully burnt and scalded, and bleeding from fearful gashes on their heads and other parts of their bodies. The boiler was a Cornish one, and had not been in use any length of time. It is stated that the pressure at which it had been worked during the day was from 40 lbs. to 50 lbs. the square inch, considerably below that at which it had been worked, while it had been tested to a pressure equal to 100 lbs. The safety-valve, and the other gear which would have assisted in arriving at the true pressure, appear to have been carried away, and it is feared that there will be some difficulty in eliciting the truth, as all those who could have given the information have fallen victims.

For the Journal of the Franklin Institute.

A JAPANESE AND ENGLISH CALENDAR,

Arranged for the year 1858-59, *by* S. MISIMA, *a Japanese, one of the Government Interpreters at Nagasaki.*

ALMANAC (Japanese and English) in the Year 5th Ansai (1858).

1.		2.		3.		4.		5.		6.		7.		8.		9.		10.		11.		12.	
Sio-gooath.		Ni-gooath.		San-gooath.		Si-gooath.		Go-gooath.		Rock-gooath.		Hechu-gooath.		Hachu-gooath.		Koo-gooath.		Ju-gooath.		Juits-gooath.		Juni-gooath.	
Jap.	Eng.	Jap.	Eng.	Jap.	Eng.	Jap.	Eng.	Jap.	Eng.	Jap.	Eng	Jap.	Eng.	Jap.	Eng.	Jap.	Eng	Jap.	Eng.	Jap.	Eng.	Jap.	Eng.
1	14 February.	1	15	1	14	1	13	1	11	1	11	1	9	1	7	1	7	1	6	1	5	1	4
2	15	2	16	2	15	2	14	2	12	2	12	2	10	2	8	2	8	2	7	2	6	2	5
3	16	3	17	3	16	3	15	3	13	3	13	3	11	3	9	3	9	3	8	3	7	3	6
4	17	4	18	4	17	4	16	4	14	4	14	4	12	4	10	4	10	4	9	4	8	4	7
5	18	5	19	5	18	5	17	5	15	5	15	5	13	5	11	5	11	5	10	5	9	5	8
6	19	6	20	6	19	6	18	6	16	6	16	6	14	6	12	6	12	6	11	6	10	6	9
7	20	7	21	7	20	7	19	7	17	7	17	7	15	7	13	7	13	7	12	7	11	7	10
8	21	8	22	8	21	8	20	8	18	8	18	8	16	8	14	8	14	8	13	8	12	8	11
9	22	9	23	9	22	9	21	9	19	9	19	9	17	9	15	9	15	9	14	9	13	9	12
10	23	10	24	10	23	10	22	10	20	10	20	10	18	10	16	10	16	10	15	10	14	10	13
11	24	11	25	11	24	11	23	11	21	11	21	11	19	11	17	11	17	11	16	11	15	11	14
12	25	12	26	12	25	12	24	12	22	12	22	12	20	12	18	12	18	12	17	12	16	12	15
13	26	13	27	13	26	13	25	13	23	13	23	13	21	13	19	13	19	13	18	13	17	13	16
14	27	14	28	14	27	14	26	14	24	14	24	14	22	14	20	14	20	14	19	14	18	14	17
15	28	15	29	15	28	15	27	15	25	15	25	15	23	15	21	15	21	15	20	15	19	15	18
16	1 March.	16	30	16	29	16	28	16	26	16	26	16	24	16	22	16	22	16	21	16	20	16	19
17	2	17	31	17	30	17	29	17	27	17	27	17	25	17	23	17	23	17	22	17	21	17	20
18	3	18	1 April.	18	1 May.	18	30	18	28	18	28	18	26	18	24	18	24	18	23	18	22	18	21
19	4	19	2	19	2	19	31	19	29	19	29	19	27	19	25	19	25	19	24	19	23	19	22
20	5	20	3	20	3	20	1 June.	20	30	20	30	20	28	20	26	20	26	20	25	20	24	20	23
21	6	21	4	21	4	21	2	21	1 July.	21	31	21	29	21	27	21	27	21	26	21	25	21	24
22	7	22	5	22	5	22	3	22	2	22	1 August.	22	30	22	28	22	28	22	27	22	26	22	25
23	8	23	6	23	6	23	4	23	3	23	2	23	31	23	29	23	29	23	28	23	27	23	26
24	9	24	7	24	7	24	5	24	4	24	3	24	1 September.	24	30	24	30	24	29	24	28	24	27
25	10	25	8	25	8	25	6	25	5	25	4	25	2	25	1 October.	25	31	25	30 December.	25	29	25	28
26	11	26	9	26	9	26	7	26	6	26	5	26	3	26	2	26	1 November.	26	1	26	30	26	29
27	12	27	10	27	10	27	8	27	7	27	6	27	4	27	3	27	2	27	2	27	31 Jan. '59.	27	30
28	13	28	11	28	11	28	9	28	8	28	7	28	5	28	4	28	3	28	3	28	1	28	31
29	14	29	12	29	12	29	10	29	9	29	8	29	6	29	5	29	4	29	4	29	2	29	1 Feb.
		30	13					30	10					30	6	30	5			30	3	30	2

Explanation of the Calendar, with some Remarks upon the Japanese Division of Time.

In the foregoing calendar it will be observed that the columns are numbered 1, 2, 3, 4, &c., beginning at the left hand, each column being divided. The figures to the *left* of the dividing line represent the Japanese day of the month, whilst those to the *right* represent the corresponding dates according to English computation.

The calendar commences at the left hand upper corner with February 1st, 1858, according to the Japanese computation, which agrees with our 14th of the same month. At the lower end of column 11, begins January 1st, 1859, and nearly opposite, in column 12, begins February 1st, from which may be constructed a complete calendar for that year. All that is necessary to be borne in mind, is that one-half of the months in the year have 30 days each, and the other half 29, making the total number of days in their year 354.

The Japanese day is divided into twelve divisions, the first six (sunlight) divisions beginning at sunrise and ending at sunset, and the second six (night) divisions commencing at sunset and ending at sunrise.

As the subdivisions are unequal and are constantly changing, it would seem to suggest an impossibility of constructing an instrument to indicate them equally. Such, however, is not the case absolutely, as will be seen from the following description of their indicators or clocks. These instruments are in shape somewhat like the old-fashioned eight-day hall clocks, and are generally from twelve to sixteen inches long, two and a half inches wide, and two inches through. The upper portion for three or four inches in length is enlarged to receive the works, the remainder answering for a case, as well as a guide, for the weight or motor, which is wound up by a cord, &c., like the ordinary clock.

There is arranged in a slot in the front of the case a series of metallic numbers from 1 to 6, counting from top to centre, and from centre to bottom. These are duplicated, and so fixed as to be separated or drawn together as circumstances of changing seasons may require. To the weight is fixed a pin projecting from the case sufficiently far to admit of a pointer or index hand, which descends with the weight, and marks the beginning, duration, and termination of each subdivision.

During the longest days of summer, when there are some 15 hours from sunrise to sunset, the numbers are set apart sufficiently far to require about 2·5 hours to travel a single sunlight division, while the night division of the same 24 hours requires but 1·5 hours.

In mid-winter, or what is termed the short days, the reverse of the above obtains—the night subdivisions become longer, and the day shorter.

This system will no doubt soon give way to our method, as English watches and clocks are being rapidly introduced into Japan.

The Japanese new year commences on a day corresponding to our 23d of January. S.

For the Journal of the Franklin Institute.

On the Erie Experiments on Steam Expansion. By SAMUEL McELROY, C. E.

In the paper which was published on this subject in the October and November numbers of this *Journal*, objection was made to the conclusions of the Board of Naval Engineers, as to the principle, the method, and the results of its experiments. Although the report was received in April, through the courtesy of Engineer in Chief Isherwood, towards whom, as well as other members of the Board with whom I have been in former times associated, no differences of professional opinion can affect my esteem as an engineer or my regard as a friend, it was with much reluctance and after long delay, in the hope that some other gentleman would meet the obvious necessity of a formal and public protest against its assertions, that I completed the notes made at the time of its first perusal, and placed them on record.

It was due to these gentlemen by every law of etiquette, that such a protest should be made with the carefulness, the analysis, and the dignity which characterize all investigations prompted by an educated regard for scientific truth, and this was felt to be particularly required, in view of the pointed and unequivocal manner in which matters of time-honored faith and practice were rejected and contravened.

Beyond such a formal protest it was not my intention to pass. My daily engagements, like those of all my brethren in harness, scarcely yield the leisure, if I had any personal inclination, to spend much time in what seems to me a retracing of the old formulæ, geometrical and arithmetical, which prove "twice two to be four." It is enough to have denied the premises of the challenge, which now demands me to believe and figure otherwise, and to have done this with sufficient elaboration of the arguments of my faith.

There is, however, in the reply of a member of the Board, in the December number, to this protest, enough of admission and explanation, to enable us to sum up the matter without much discussion, and without involving a controversy in which I am not at liberty to indulge; and I therefore propose to notice this reply as to a point or two presented.

In discussing the principles of experiment violated at Erie, it was claimed generally that the problem in which all engineers are directly interested "is defined by the amount of work to be done, and the most economical method of doing it," and consequently that the real matter at issue is—"whether it is cheaper to carry high steam and expand, or low steam and follow full stroke." As the Board maintained the same steam pressure, and reduced the amount of work done to suit the various grades of expansion tested, we claimed that the benefits of increased pressure were denied the measures of expansion, on this general principle.

Mr. Stimers replies to this that we do not "appear to be conscious

that this mode of regarding the subject unites the question of high pressure with that of expansion; whereas the experiments were instituted, not to ascertain the relative economy of using steam of different pressures, but "the relative economy of using steam with different measures of expansion."

He then quotes a page or so of the report, to show that "they recognised fully and stated clearly, the advantages of high pressure steam." So far then as the question of practical operation is at issue, it is here admitted, that the results would have been much more favorable to the increasing grades of expansion, if the Board had handled the *Michigan's* cylinder, with a regard for economical results, and in the manner adopted by educated men with a view to such results. A very important distinction is therefore drawn between an abstract question under experiment and a question of absolute economy.

But Mr. Stimers forgets in this admission, that the report which he signed, distinctly denies that greater economy in result would have been thus realized. On pp. 33 and 34, a case is presented which "involves not only the effect of different measures of expansion *per se*, but of different initial cylinder pressures." For such a case as this, the following argument is given:

"The [tabular] numbers which express the comparative economic efficiency of the different measures of expansion in rapport of total power, will also express their comparative economic efficiency in rapport of net power when the initial cylinder pressure is adapted to give the same net pressure with the different measures of expansion. Hence the quantities on line 21 of table No. 2, express the comparative economic efficiency of the different measures of expansion in rapport of fuel, when the same engine is employed with the same velocity of piston and net pressure upon it. [Line 21 gives these quantities: 11-12ths cut off 1·000, 4-45ths 0·915, 7-10ths 0·882, ½th 0·877, 3-10ths 0·874, ¼th 0·852, 4-9ths 0·840.] This deduction is rigorously exact, if we except the modification due to the difference of temperature in the cylinder during a stroke of the piston; but the correction for this, whatever may be its amount, will be in favor of the *less measures* of expansion, because, as with them, in order to obtain the same net pressure on the piston, the initial cylinder pressure, and consequent temperature of the entering steam, will be much less, while the temperature of the back pressure remains constant, the difference will be much decreased, and the condensation due to these extremes proportionally lessened."

Whatever, then, may have been the preliminary arguments of the Board, its summing up of results sacrifices consistency, and condemns the very clear and intelligent admission of one of its members, while at the same time the particular line, 21, referred to, gives us a striking illustration of the anomalous results obtained through the erroneous method of experiment adopted.

To the objection also made on the score of principle, that the Board could not reduce the coal combustion from 18·52 pounds to 3·79 per sq. ft., the engine load from 29·8 to 8·8 lbs. per sq. in. of piston, and the revolutions from 20·6 to 11·17, without violating the established doctrine of maximum useful effect, we have no answer, and therefore credit Mr. Stimers with a negative assent.

In place of this we have an argument to show that all the objections in detail, which cluster around this violated principle, do not influence "in the slightest degree, the only measure of the cost of the power

developed by the engine," which is pronounced correct by the Board, and that is the weight of water by tank measurement, consumed per hour per horse power. This means to say then that it can make no difference to the economy of an engine, how much coal is wasted by imperfect combustion, or how much water is wasted by perceptible and imperceptible leakage, by priming, and otherwise, or how much violence is done the development of power in the steam produced from the water evaporated. All these sources of error and loss are merged in the asserted infallibility of the tank measurements, which in these experiments have a range of difference from the record of the indicator from 3 to 37 per cent. And without being able to deny the accuracy of the indicator, it is a singular fact, that the Board should have been willing to endorse the tank so implicitly.

To the objection that the results of Experiments Nos. 1 and 2 are combined, because no interval elapsed between them, we are told that "there was nothing to be changed except the damper in the smoke-pipe and the point of cutting off." This admission sufficiently illustrates the reliance which is to be placed on the distinct coal accounts given, and the process of changes in coal combustion and engine resistances; and it is a singular comment on the necessity otherwise suggested of occupying *several hours* to adjust the engine to the *normal conditions* of each experiment.

Nor is our assertion contradicted, that the coal account for each experiment is not accurately determined. The possibilities of error are fully admitted, on the plea that they could not be very important. Whatever may be the per centage of error at issue, the question of correctness in method has an importance far superior to any single detail in result.

As to the question of back pressure, it is a sufficient admission that one of its prominent measures is the "weight of steam discharged at the end of the stroke." In these experiments, prejudicial as we have shown them to be to the available benefits of expansion, we find on the basis of the comparative work done, that between 11-12ths and 4-45ths cut-off, while the actual weight of steam used, per indicator, was in the proportion of 966 to 146, the work done was as 343·9 to 86·5, whereas the proportionate work for the shorter cut-off should be but 52, showing that about 40 per cent. less steam would meet the ratio of work, and by consequence that there is no propriety whatever in assuming a common measure of back pressure for different measures of expansion.

The explanation of the friction of the load, which was taken at 2·1 pounds per square inch of piston, whether the load was 29·2 or 7·7 pounds, and whether the speed of the wheels was 20·6 or 11·16 revolutions, on the ground that 2·1 pounds represent the "constant pressure" caused by the "gravity of the different parts of the engine," introduced as an element of comparative measure for different expansions, is still far from clear to our "looseness" of observation. It is not a supposable case, that the parts of an engine are at any time in-

dependent of their load and speed, so that a uniform resistance may be attributed to all variations of either.

Nor can we understand that boilers at work under pressure, can possibly be in the same condition of protection from leakage as when standing cold. Mr. Isherwood's experiments on the *Prosser Boiler* showed an imperceptible leakage of 7·89 per cent., and all boilers under steam must waste water, more or less, until they are made of something less porous than heated iron and expanding tube sheets.

All these matters of detail in experiment, and the questions of back pressure, frictional resistance, indicator diagrams, and the like, in none of which is the report properly sustained, are, however, of minor consequence when compared with the main admissions we have noticed; and, in fact, the entire tenor of this reply is summed up in the following singular apology:—

"The careful reader will observe as very prominent in the foregoing quotation from the report of the Board, that it was not endeavoring to ascertain how much it was desirable to expand the steam in *existing engines*, but in those which were yet *to be built*, and in which proportions could be given that would do the required work in the most economical manner."

"It will be observed in this quotation from the report, how completely the Board sank all considerations of *existing engines* in its search for the true principles which should govern *future constructions*."

Precisely what kind of a construction this new style of engine is to be, the report does not disclose; but it is clear, at all events, that we have been right in denying the correctness of its conclusions so far as they affect the several classes of engines now in use, or formerly used. When the new motor appears, we are to find that it is more effective under low steam than under high steam, and under a throttle in place of a cut-off; that its back pressure shall be always a minimum under all conditions of service; that the friction caused by the gravity of its parts shall be constant for all work; that it shall work equally well and as economically under light or heavy loads, at slow or quick speeds, and with any variations of coal combustion; and, last of all, that it shall be independent of the mechanical forces which, by inertia and momentum, now control the doctrine of expansion. To such an engine as this, and not to the *Michigan's*, the elaborate tables of this report prophetically apply. All this is very well, and in the meantime the engineering world will be permitted to exercise its faith in Mariotte, and Morin, and Weisbach, as usual.

This explanation solves a difficulty which otherwise affected us. Having had occasion in engine designs for a very important work to examine all the experiments or records within reach as to certain classes of engines, including the "double cylinder," we had noticed among various other testimonies, a paper on *Double Cylinder Marine Expansion Engines*, in the *July* number of this *Journal* for 1860, in which the writer uses the following language generally, and particularly towards Mr. Isherwood's *Precedents*:—

"From this table, it would appear that where the heat of the steam is neither increased nor diminished by extraneous influences, any *expansion* of the steam causes it to be

superheated, and not condensed, as some have erroneously asserted, basing their assertion upon the laws of steam as given by Regnault.

"A little reflection must convince any one not carried away by a kind of *scientific fanaticism*, that the yielding of the piston in the cylinder to the pressure upon it affects the steam only by giving it an opportunity to expand. What, then, becomes of Prof. *Joule's* theory that condensation is caused by a rendition of power on the part of the steam? It is painful to know that such absurd doctrines are advanced by men of learning and position, and that they are proved to be correct, and adopted into the calculations of others equally learned and high in position."

The writer discusses this matter at considerable length, showing the absolute gain by expansion *in the cylinder*, and the absolute gain of using a combined engine which is obliged to expand three times any way, and cuts off on the main valve besides at one-quarter.

Whatever difficulty we might have had in reconciling the emphatic and *demonstrated* argument of this paper, with the equally emphatic and directly opposing argument of the Erie Board, Mr. Stimers happily relieves by this opportune explanation, which now informs us that all the Erie discussions are to be taken in a Pickwickian sense, as to all present classes of engines. We were entirely in the right to have demonstrated the "absurd doctrine" of the Erie Board on the basis of present practice, and have only erred in our inability to eliminate from the report the latent prophecy of a new form of motor, to which it solely applies. Mr. Stimers' correction will, therefore, prove very serviceable to the "large number in the profession" who read professional papers with reprehensible "looseness," and could not otherwise appreciate the profound depths of the report. They will undoubtedly express their regrets that he did not by a foot note, or in some other plain and reasonable manner, append this context to that document before signing it.

Having extended our examination of this application of high measures of expansion by double cylinder engines into an elaborate research, assisted by actual experiments of a close and satisfactory character, we found it very clearly established at last, that the same benefits could be attained much more advantageously in single cylinders properly arranged; and we therefore suggest to this gentleman a continuation of the same research, in order that he may be, if possible, yet more positive in his denial of the *Joule* theory, and may continue in the consistency of his faith as to the present steam engine —mythical engines and abstract calculations not being gifted with a due modicum of horse-power for immediate work.

Without pausing to congratulate him on the admirable manner in which we are thus enabled to preserve our respect for his consistency in a profession where demonstration is absolute when in accordance with plain, natural, mechanical laws, and in a case where inconsistency would be fatal to his reputation as a *practical* man, we proceed to express our pleasure at finding from his remarks that the great and fundamental principle of all engine motion, which we referred to, has sufficiently attracted his attention to induce a trip or two in New York Harbor on the *Richard Stockton*, to study her engine in motion.

Possibly from a somewhat hasty reading of our suggestion, he seems

to have understood us to say, that "as the *inertia* of the mass—the motion of which is accelerated during the first half of the stroke and retarded during the last half—absorbs a portion of the power of the steam while being accelerated, and returns it again while being retarded, the engine works more smoothly and properly when cutting off at less than half stroke than when following beyond that point." This he calls "visionary speculation."

Now we did not assert that the *inertia* was either accelerated or retarded, not being aware that the idea of motion could properly be associated with inertia; that would be a "visionary speculation" of course, unless Mr. Stimers was inadvertently recalling that proposed novelty which may possibly possess inertia in some other state than that of rest. Nor did we say anything about an engine working "more smoothly and properly" with less than half cut off. What we did say was this:—

"To overcome the inertia of an engine, a certain surplus pressure must be applied to the piston, which corresponds with initial pressure, and is exceeded at no after point of the stroke. The mass being thus put in motion by charging it with surplus power, it is a mechanical absurdity to continue the initial pressure any further than will suffice to complete the stroke by virtue of the surplus power imparted at the commencement."

We remarked in connexion with this, that by this principle, "we come back again to the doctrine of maximum useful effect," which apprehends both load and velocity; and we also said that the "maximum velocity of motion" could be imparted to an engine before it reaches the half stroke.

If Mr. Stimers means to say that this theory of engine motion is a "visionary speculation," and has commenced sundry excursional trips to obtain evidence on the subject, arguing his correctness from the first trial because he found a certain case of high speed incompatible with high expansion, we shall be happy to refer him to a very large class of high speed engines, which do cut off very short with great economy, and then to a class of low speed engines which also cut off very close, both classes being entirely consistent with the doctrine urged, and respectfully suggest that maximum useful effect involves the element of load as well as velocity. It is also readily demonstrable that a high range of economical result depends not only on thoroughly educated design, but on thorough scientific management. We have had considerable experience of the difference between an engine carefully designed to fulfil a certain purpose, and the management of the same engine in the hands of what we used to call in "our date," "intensely practical men," a class entirely educated through their fingers and thumbs, and who, by the way, are particularly vindictive towards any other school than their own.

With regard to this matter of engine motion, we cannot but feel that Mr. Stimers has taken precisely the right course. Close study of all results heretofore obtained, sifted and digested by applying the recognised laws of mechanical forces, in connexion with accurate observation and experiment, will always guide an unprejudiced student into the path of true practice. And pursuing this commendable course,

we have no doubt that in less time than elapsed between the contribution to the *Journal of the Franklin Institute* which we have noticed, on *Double Cylinder Engines*, and the compilation of the *Erie Report*, this gentleman will fully comprehend how rigidly correct we are, as to the present steam engine, in declaring that "the idea of assuming full steam travel as a basis of comparative mechanical action, is a misapprehension of engine duty."

This sums up, I believe, all the questions at issue of paramount importance. As to the benefits of higher steam in connexion with expansion, and as to all the other questions of principle and method which characterize these experiments, all that I claimed appears to be sufficiently admitted in relation to all *existing engines*. I have nothing to offer prematurely about prophetic combinations; these may be freely conceded to the new school of speculation and design.

There is but one thing more to say, and that is necessarily brief. This member of the Board occupies the first page and other portions of his remarks in asserting my incompetency to "comprehend" the report, while indulging in a sneer at my "boldness, intelligence, and literary ability;" also crediting me with "slight practical experience," apparently with the object of disarming my criticism. In that comprehensive school of the profession which I left to enter the Engineer Corps of the Navy to accomplish a certain purpose, and to which I returned after my resignation, a resort to such personal arguments is considered a confession of judgment. For the sake of all that is courteous among gentlemen, and dignified in scientific discussion, and truthful, I hope I am correct in supposing that these things were written in haste and have been regretted at leisure. One thing is certain, and this is, that nothing further of the kind will be noticed in the *Journal of the Franklin Institute*.

For the Journal of the Franklin Institute.

Construction of Arcs of Circles having Large Radii, the Versed Sine being Unknown. By J. K. Whilldin, Civ. Eng.

The following method of constructing arcs of large circles, I have found to be very convenient, especially when making full size drawings of details attached to large cylinders and other circular objects.

When beam compasses exceed five or six feet in length, they become rather clumsy to manage with ordinary drawing boards, and it becomes necessary in laying down arcs of large circles to obtain certain points in the desired curve, and trace a line through them by means of a batter and weights.

With the ordinary modes of constructing these curves, the versed sine is assumed as *known*, but it frequently happens that this item is unknown, and we have only the value of the radius as a basis of operations. True, we may calculate* with this datum the versed sine corresponding to any assumed chord, but this is a tedious process, and

* Versed sine = Radius $- \sqrt{\text{Radius}^2 - \left(\frac{\text{Chord}}{2}\right)^2}$.

with very large radii and small chords, unless we extend the operation to many places of decimals, we do not obtain any considerable accuracy.

The advantage of the method here given is, that while it requires no calculations, the points obtained by it are as correct as those which would be found by the beam compass, could the latter be used.

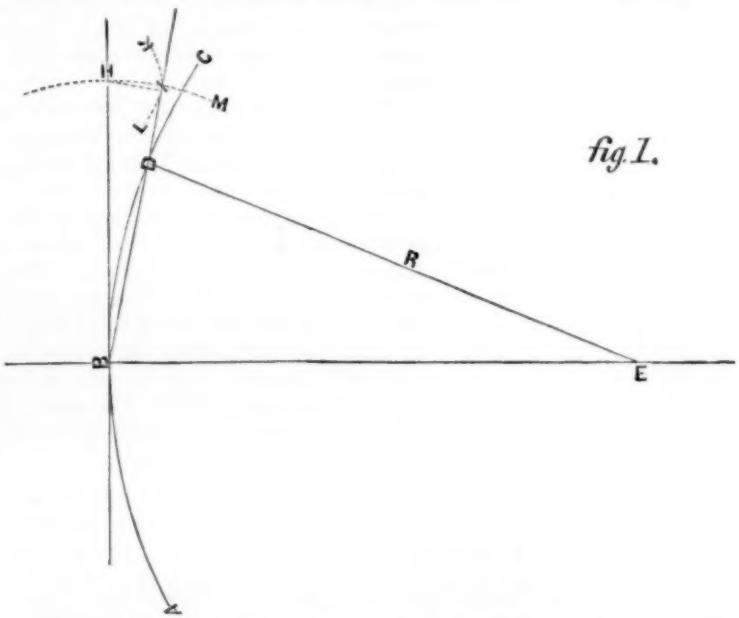

fig. 1.

Let A B C be the arc we desire to construct, R the known radius; with the radius R taken on any convenient scale of equal parts, describe the indefinite arc H M from the point B; also take $\frac{1}{2}$ of any assumed chord B D on the same scale as R, and describe the arc L I K from the point H; now from the point B draw a line tangent to the arc L I K, and on the line thus drawn lay off from the point B the assumed chord B D. Then will the point D be in the arc sought, and the curve may be run through it, and other points found in the same manner, by means of a batten.

Example.—Suppose R = 10 feet, and suppose we take a chord B D = 14 inches.

We will begin by taking R, say on a scale of $1\frac{1}{2}$ ins. to the foot, which will give us 15 ins. as a radius to describe an arc H M from the point B. Again taking $\frac{1}{2}$ of 14 ins. (the assumed chord) on the same scale, we have $\frac{7}{8}$ths of an inch as a radius to describe the arc L I K from the point H; drawing the line B I, and laying off on it from the point B the actual length of the chord B D = 14 ins., we obtain the point D through which to run the curve.

The foregoing construction is based on the well-known fact that the

angle H B D contained between the chord and tangent is one-half of the angle B E D, which is measured by the chord B D; and the taking of the radius R and $\frac{1}{2}$ the chord B D on a scale of equal parts, is merely a convenient mode of constructing the angle H B D.

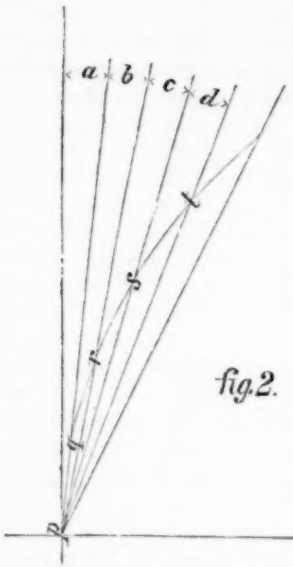

fig. 2.

The inclination of the chord being thus found, and its length B D being already known, the point D is therefore determined.

The angle H B D may also be constructed by taking the chord and the diameter of the large circle, both on the same scale of equal parts.

Railway curves are sometimes projected by methods analogous to the above by means of the transit and chain. Thus, in fig. 2, if *a*, *b*, *c*, &c., be equal angles, and we commence at the point *p* and lay off the equal distances *pq*, *qr*, *rs*, &c., then will the points *p*, *q*, *r*, *s*, &c., be points in a circle, whose magnitude depends on the equal measures *pq*, *qr*, &c., or, if these be constant, on the equal angles *a*, *b*, *c*, &c.

Washington, Dec., 1861.

For the Journal of the Franklin Institute.

On Electrical Machines. By E. S. Ritchie.

The electrical machine known as the Ruhmkorff Induction Coil, resulted from the researches and discoveries of Faraday, Henry, De la Rive, Fizeau, and others.

Mr. Ruhmkorff of Paris was the first to construct this instrument, following the arrangement of Faraday, surrounding an electro-magnet by a helix of insulated copper wire, to which he applied the automatic interrupter of De la Rive.

The important discovery of the action of the condenser to the interrupter, by which the intensity of the induced current is increased so as to pass an interval, is due to Fizeau, and was immediately adopted by Ruhmkorff.

The limit which Ruhmkorff attained in his most powerful coils was to throw the spark less than one inch.

Mr. Hearder, in 1857, improved the apparatus by more carefully insulating the helices, and obtained sparks of three inches.

During the spring of 1857, I attempted the construction of the instrument, following the general form adopted by Ruhmkorff; but, beyond the narrow limit already attained, I found it impossible to make one which would not destroy itself by the discharge taking place within the helix, between the outer and inner portions, which

were necessarily brought in near proximity, depending alone upon insulating media.

I saw that this was an insuperable difficulty unless a mode of construction could be devised by which the portions of the helix should be separated by a distance nearly equal to the tension of the electricity induced in the wire. I wound a coil with the strata laid in conical form, each course beginning upon the bobbin, and winding up a cone of 45° until the diameter of the exterior was attained; then an insulation of rubber was overlaid, and another course wound in like manner, and so on: the experiment proved successful. I then devised a mode of winding the wire in planes perpendicular to the axis, alternately running from the inner to the outer and from the outer to the inner diameters of the helix, insulating between the strata; and, in July, 1857, instruments were made giving sparks twelve inches in length.

Prof. Wm. B. Rogers, at the meeting of the British Association held in Dublin in 1857, stated the power of my coils, and soon afterwards I received from John P. Gassiot, V. P. R. S., and from Prof. Forbes of the University of Edinburgh, orders for instruments of twelve and nine inches, which were sent them the following spring.

Until July, 1859, no instruments had been made by Ruhmkorff of greater power than three or four inches in length of spark, when he received one of my coils, which he dissected and copied, as will be evidenced by the following

Statements by Prof. MacCulloch.

1. In the summer of 1858, I ordered of Mr. Ruhmkorff one of his induction coils, similar to that for which the Academy had awarded him a prize. In my letter, I stated that Mr. Ritchie professed himself ready to surpass whatever might be its power, and mentioned that he had made for Columbia College a coil giving twelve inches sparks, with a pile of four carbon cells. In the reply of Mr. Haskell (my Paris correspondent) it was stated that Ruhmkorff expressed surprise that this result had been obtained from four cells only. He did not accept the challenge, but agreed to make one of his best and most powerful coils with other miscellaneous apparatus.

2. In May, 1859, having obtained nothing from Mr. Ruhmkorff, I visited him in Paris, and urged the prompt execution of my order; he considered my request that he should send to New York a machine to be surpassed by one of a competitor as an unfair proposition. To this I replied that I would request Mr. Ritchie to send to Paris the most powerful coil he could make. With reference to my order for a large coil, he showed me the wreck of the only one he had attempted to make, and informed me that it had destroyed itself by the spark passing through the coil breaking down the insulation, &c., and he was unwilling to make any but his ordinary coils (price 300 francs), which, however, with other apparatus, he agreed to furnish promptly.

3. Supposing that Mr. Ruhmkorff would be happy to compare the coil of Ritchie with his own and that of Hearder, desiring to gratify him, and not for an instant doubting that he would accord to Ritchie all that is peculiar in his perpendicular system of winding the second-

ary bobbin, I presented to Mr. Ruhmkorff, in July, 1859, one of Ritchie's coils giving sparks of seven inches, which he dissected, for the purpose of examining its construction.

4. About the same time, I ordered from Mr. Ritchie the most powerful coil he could make, and requested him to send it to Paris. This instrument arrived in September, but was in the hands of the Custom House officers until November. When received it was taken to Mr. Duboscq's, who politely offered me the requisite facilities for experimenting with it. It was tried in the presence of Mr. Jacobi, of St. Petersburg, and of MM. Foucault, Lipajous, Nachet, and Duboscq. Afterwards it was exhibited privately to MM. Jamin, Sénanmont, Verdet, Desains, Froment, Du Moncel, Baron Thénard, Gaverret, and others, and publicly to the students of the École de Médecine. By request of M. Jamin, it was also shown to the general commanding and several of the professors of the Polytechnic school.

5. During the month of February, 1860, I met M. Jamin, who told me that the Polytechnic school had for more than two years urged M. Ruhmkorff in vain to make for its cabinet a powerful coil; that in the month of March it would be necessary to close the account of expenditures for the current fiscal year, and that unless Ruhmkorff should then deliver the long-solicited coil, the Polytechnic school would be glad to buy that of Mr. Ritchie. In the month of March, Mr. Ruhmkorff delivered to the Polytechnic school the coil ordered from him.

6. In May I saw the said coil at the Polytechnic school, and was informed by M. Jamin that the secondary bobbin is wound in portions perpendicular to its axis (Ritchie's system); also that it gave sparks of 33 to 40 centimetres (13 to 16 inches), about the same as those of Mr. Ritchie's coil.

7. In June, Mr. Ruhmkorff showed me in action a coil just finished by him for M. Du Moncel, a duplicate of that of the Polytechnic school. The sparks were of about the average length of those from the Ritchie coil. Mr. Ruhmkorff gave me as the length of the sparks for both the coil of the Polytechnic school and that of M. Du Moncel, 33 to 40 centimetres; and he stated that the secondary bobbin is divided (fractionné) perpendicular to the axis. * * * *

(Signed) R. S. MacCulloch.

New York, September 8th, 1860.

The French scientific journals immediately noticed the instruments made by Mr. Ruhmkorff, awarding to him the credit of all the improvements made. The Vte. Du Moncel, Ganot, and other French writers who have published descriptions of the instrument, though fully conversant with the facts, have carefully ignored them. Prof. MacCulloch wrote to Vte. Du Moncel to correct statements in the *Cosmos* which he considered unjust to me, but no notice was taken of his request.

I am induced to make this statement only because my silence has been construed into an admission that I had made claims which I could not substantiate.

Boston, December 15th, 1861.

New Fusible Alloys: Remarks on determining the Melting Point of Metals, &c. By B. Wood, M. D.

To the Editor of the Journal of the Franklin Institute.

Seeing that the "fusible metal" described by me some time ago, (consisting of cadmium 1 to 2 parts, tin 2 parts, lead 4 parts, bismuth 7 to 8 parts, and melting between 150° and 160° Fah.,) has been viewed with some interest by men of science, I take the liberty to offer through your *Journal* a short description of another alloy, which perhaps has equal claims to scientific notice. It consists of

Cadmium 1 part, Lead 6 parts, Bismuth 7 parts.

This alloy melts at about 180° Fahr., being nearly midway between the melting point of the old fusible metal, consisting of the *three* metals, tin, lead, and bismuth, and that of the alloy above mentioned, consisting of the *four* metals, cadmium, tin, lead, and bismuth. It is remarkable as exhibiting the fluidifying property of cadmium in certain combinations—(a property to which attention was directed in vol. xl. No. 2, of this *Journal;*) and also in the fact that, while the mean melting point of the constituents composing it is much higher than that of those composing the old fusible metal discovered by Newton, (being in the former case about 540°, in the latter 500°,*) it melts at a much lower temperature. It is more fusible than any other *alloy* yet known consisting of but *three* metals.

It appears to be the most fusible form of mixtures of cadmium, lead, and bismuth, any material variations from the proportions stated resulting in a higher melting point. Thus, taking the extremes on either side, a mixture of 1 part of cadmium, 1 of lead, and 2 parts of bismuth, barely softens in boiling water, while one of 1 part cadmium, 10 parts lead, and 11 bismuth, becomes fairly soft. A mixture of 1 cadmium, 6 lead, and 6 bismuth, barely *melts* or fluidifies in the same, and of 1 cadmium, 6 lead, and 8 bismuth, becomes fairly fluid. From these points the fusibility is increased or diminished as we approach to or recede from the proportions of the formula.

The alloy presents a clear, brilliant, metallic lustre. It does not tarnish readily; whereas the combinations of lead and bismuth are remarkable for the facility with which they not only tarnish, but oxidate upon the surface. Its color is what would be denominated "white," but more strictly a bright bluish grey, very nearly resembling platinum in hue and lustre. When cast, its free surface presents a white, frosted appearance. It is very flexible in thin plates, and the fracture is hackley, but when broken in thicker bars the fracture is smooth, resembling that of tempered steel. It is malleable, although not perfectly so, cracking upon being reduced one-third to one-half of its thickness. In hardness it is the same as bismuth, or nearly the same as an alloy of 2 parts of lead and 1 part of tin (coarse solder), which it resembles more nearly in other respects.

* Taking the melting point of tin as 440°, of bismuth 480°, lead 600°, and cadmium 600° (being the same as lead, notwithstanding it is placed in most of the books as low as 442°), then the mean of 1 part tin, 1 lead, 2 bismuth, is 500°; or of 3 tin, 5 lead, 8 bismuth, 510°; the mean of 1 cadmium, 6 lead, 7 bismuth, = 540°. That of 1 cadmium, 1 tin, 2 lead, 4 bismuth, 520°; or of 1½ cadmium, 2 tin, 4 lead, 7½ bismuth, 518°.

It may be that more approved methods of measuring temperature will give the alloy a still lower melting point than above ascribed to it.

I see by a notice in the June number of this *Journal*, copied from a European publication, that Lipowitz gives my "fusible metal" a much lower melting point than I had indicated. He found, it is stated, that an alloy composed of 3 parts cadmium, 4 tin, 8 lead, and 15 bismuth, "softens between 131° and 140° Fahr., and near 140° becomes perfectly fluid;" whereas, finding the most fusible formula to become fluid so as to be fit for use in casting, at 160° F., and to harden at 150°, I designated its fusibility as "between 150° and 160°." Of different variations the proportions of the following formulas, or proportions intermediate between the two, give the lowest melting point, which appears to be identically the same in either case, being 150°, or very nearly, viz:

Cadmium 1 part, Tin 1 part, Lead 2 parts, Bismuth, 4 parts,
or " 3 parts, " 4 parts, " 8 " " 15 "

Determining the melting point of these by means of a mercurial thermometer of high range, and not graduated to indicate fractions of degrees, the bulb being immersed one-fourth of an inch in the melted metal contained in a small porcelain crucible, it was observed that, on cooling, the metal was fluid around the bulb at 150°; at 146° or 147° it was still soft; the mercury then *rose* to 150°, and after remaining stationary there some moments the metal congealed completely, after which the mercury still remained at the same point for some moments before it began to fall. In the proportions of 1 part of cadmium, 2 parts of tin, 4 of lead, and 7 of bismuth, the alloy congeals at 154°.

There are difficulties in the way of exactitude in measuring the melting point, and we find a want of uniformity in the results of different experimenters, owing partly, perhaps, to imperfections in our instruments, partly to the different modes of using them, and partly to the want of a uniform rule and standard of measurement. What *is* the "melting point"? or what is the precise condition of a metal when it is said to "melt"? Are we to reckon from the point at which it softens? or fluidifies? or congeals? Some alloys pass from one condition to the other at nearly the same temperature, while with others there is a difference of fifty degrees or more. There is generally, too, a difference between the point at which metals *become* soft or fluid in heating, and that at which they *remain* fluid or soft on cooling, however carefully the process be conducted. Again, *how* should we measure the temperature of a metal in the melted state? Should the bulb of the thermometer be submerged, or is it sufficient to simply dip the lower extremity? If we adopt the latter mode, noting the temperature, and then plunge the bulb deeper, the mercury will be observed to rise. In taking the temperature of the atmosphere, the medium envelops both bulb and stem. When the bulb is partially immersed in a hot liquid, the upper part of the contained mercury radiates a portion of the heat received by conduction from the lower. Suspending the bulb in hot water to the depth of half an inch, the mercury in the stem

stood at 184°; while on partially withdrawing it, so that only its extremity remained in the water, the mercury fell to 170°. In other instances the difference was about 15°, more or less. The lower strata of the fluid in such cases may indeed be hotter than those near the surface, as receiving the heat sooner and parting with it later: but this argues a difference of temperature in the upper and lower strata of mercury in the thermometer bulb when placed in like situation, and if it were long enough the effect of the heat acting upon the lower extremity might be neutralized by cold applications to the upper. A piece of metal may be held between the fingers by one end while it is melting at the other. It is true, metals when fluid conduct heat more readily. But if we examine different portions of a mass in this condition, we shall find marked inequalities of temperature, (nor is equilibrium restored so long as the metal remains fluid, except the whole be kept in this condition in a uniform medium.) Thus, when cooling, the outer portions part with their heat faster than they receive it from the centre, and consequently are first to congeal.

But there needs no proof to show that the indications of the thermometer will be erroneous when the greater part of the bulb is in a cooler medium than the metal under examination. The facts referred to only show that the error may be greater than is generally supposed. They also point to difficulties in the way of obtaining exactitude by the other method. But, what is more directly to the present purpose, they speak the need of uniformity in the mode of measurement, on the part of those not provided with means of obviating these difficulties, that our *approximations* to the truth may be uniform.

When the bulb is immersed in the melted metal, the lowest temperature at which it will move freely may be taken as the melting point, and that at which it adheres, as the point of congelation. The cylindrical bulb is more convenient than the globular, and it would be better still if in shape of an inverted cone.

INDIANAPOLIS, Ind., Dec. 20, 1861.

FRANKLIN INSTITUTE.

Proceedings of the Stated Monthly Meeting, December 19, 1861.

John C. Cresson, President, in the chair.

John Agnew, Vice President,
Isaac B. Garrigues, Recording Secretary, } Present.

The minutes of the last meeting were read and approved.

A letter was read from the Liverpool Literary and Philosophical Society, Liverpool, England.

Donations to the Library were received from the Royal Astronomical Society and the Chemical Society, London; Frederick Emerick, Esq., and Hon. W. D. Kelley, Washington City, and Charles Ellet, Jr., Esq., Georgetown, D. C.; and Prof. John F. Frazer, Philadelphia.

The Periodicals received in exchange for the Journal of the Institute, were laid on the table.

The Treasurer's statement of the receipts and payments for the month of October was read.

The Board of Managers and Standing Committees reported their minutes.

Candidates for membership in the Institute (9) were proposed, and the candidates proposed at the last meeting (13) were duly elected.

Nominations were made for officers, managers, and auditors for the ensuing year.

Mr. Washington Jones, Chairman of the Committee on Meetings, exhibited Amies' patent Universal Square, which combines five different instruments, viz: the try-square, the miter, the T-square, the graduated rule, and (what is entirely new) the centre square, for finding the centre of a circle. This instrument was made by J. R. Brown & Sharpe, Providence, R. I.

He next presented, from the same makers, a Centre Gauge, for adjusting the work in a turning lathe.

The same gentleman also exhibited a standard Scale of hardened steel, graduated from $\frac{1}{8}$th to $\frac{1}{100}$th of an inch by machinery, combining accuracy and cheapness. It was made by Darling & Schwartz, Bangor, Maine. All the above tools where presented by Messrs. Field & Hardie, of this city.

Mr. Jones also exhibited a Razor Strop, the invention of Mr. C. Y. Hayne, the polishing cushion of which is stuffed with sand, instead of lying flat on the body of the strop as usual.

Mr. Howson, of the Committee on Meetings, submitted a Watchman's Clock, the manufacture of Messrs. Code, Hopper & Gratz, of this city. By means of this clock, it may be readily ascertained whether a watchman has remained at his post during the proper hours. A slit is made in the face of the clock, through which the watchman introduces a pencil at stated intervals to mark a revolving dial; the marks on the dial show the hours at which he has been at his post.

Mr. H. also presented a great variety of Paper Tags and Collars, both plain, printed in colors, and embossed, made by the Lockwood Manufacturing Company of this city.

He also exhibited an apparatus, the invention of E. H. Bailey of this city, which may be readily attached to the lock of any door, to prevent the key being turned by nippers or other tools from the outside.

Mr. Howson then placed on the table a working model of H. G. Armstrong's Paper Bag Machine, as improved by S. E. Pettee; also a specimen of Photograph Album, made by Turner Hamilton.

Specimens of Envelopes made by a machine invented by Mr. Pettee were also exhibited by Mr. Howson. Envelopes made in this manner cost considerably less than those made by hand, and can be made at the rate of 30,000 per diem.

A patent Camp Trunk was exhibited by W. A. Andrews, the inventor. The lid of this trunk contains a folding bed or couch, and a table, being so constructed as to be easily removed without disturbing the other articles in the trunk.

A. L. Fleury, Esq., read the following paper on the Preservation of Stone, and some new methods of preparing a pure aqueous Solution of Silica:

The subject of the preservation of stone has recently been most

thoroughly discussed by some prominent English chemists,* and the writer thinks it is worthy of more special notice; less on account of the necessity of preserving *our* building stones, (these seem to be mostly selected with the very view for change, and we Americans have never, until perhaps very recent times, been over-anxious for the preservation of old buildings,) than of the great number of applications which we are able to make of a new material, *hydrated silica*, recommended for the preservation of stone.

The alkaline soluble silicates of soda or potassa, the water-glass of Fuchs, Kuhlmann, Liebig, and others, are too well known to be here described, and those desirous of being informed may read a very graphic description in the *American Journal of Science and Arts*, for September and October, 1861. These silicates will, however, give way to a better adapted and much purer article, to the *pure aqueous solution of silica*.

The manufacture of the alkaline silicates will not suffer thereby; on the contrary, after the real value and immense pecuniary and other advantages shall have permeated the American public, it will soon increase and develop itself on a most gigantic scale.

The progress in this department, however, is not confined to our more scientific English, French, and German neighbors; what they communicate to us as new facts, has been known for many years past in the United States. The gentlemen cognizant of these important facts, partially to escape the ever lurking harpies of speculators and patent thieves, partially to elude the attacks of some chemists, who declared it impossible for silica to exist in aqueous solution without the addition of an alkaline base, kept their own counsel, but will shortly exhibit such specimens as will far excel those of the English chemists.

The following extract from an article written by the editor of the London *Chemical News*, No. 100, p. 227, contains some valuable suggestions:

"No one who has critically examined the various projects for hardening and protecting stone, which have during the last few years been made public, can have failed to remark the great importance which, in the majority of instances, is attached to the action of silica. Thus, in reviewing the processes submitted to the Decay of Stone Committee, among the eleven proposals which have already been under consideration, no less than seven depend for their efficacy upon the action of this very substance under various conditions of employment. Advantage is taken of the mutual decomposition exerted between certain silicious compounds applied, and the materials constituting the building stone itself; or, otherwise, systems are founded upon the production of compounds from an alkaline silicate, and a soluble earthy salt, successively applied. The known properties of silica and of the class of silicates have, no doubt, powerfully contributed to the formation and establishment of this opinion. The facility and cheapness with which they can be manufactured on the large scale; their unalterability un-

* See the Report of the Committee on the Decay of the Stone of the New Palace at Westminster, London Chemical News, No. 97, p. 189; also, Jour. Frank. Inst., this number, p. 3.

der trying atmospheric influences; their all but complete indifference to energetic chemical re-agents—all point to silica as the one fit material upon which the ingenuity and experimental resources of our chemists and practical men may be expended with the greatest promise of success. Let only a sufficient amount of silica be compelled to enter and occupy the pores of the stone and incrust all the exposed surfaces; let it be employed in a state of hydration, or other suitable form, in which it may gradually combine with the earthy constituents it there meets, and with its action unfettered by saline impurities, the presence of which tends often to interfere mechanically with its successful employment, and the strong presumption is that the House of Parliament will endure, so far as the stone is concerned, as long as the Pyramids.

"The difficulty which besets many of the processes of silicification is, that along with the needful silica, so much superfluous, and, indeed, injurious matter is introduced, that the valuable qualities of the silica are in a great measure counteracted; the disintegration of the stone sometimes caused actually by the efflorescence of these extraneous substances, and the porous character necessarily induced as the consequence of the gradual removal of the soluble salts in juxtaposition with the silica undoing the binding and hardening action of this valuable material. From this train of reasoning, it became evident that if hydrated silica could but be deposited in the pores of a limestone or dolomite, without the assistance of potash or soda as a vehicle, and there be left slowly to enter into chemical union with the earthy bases of the stone, its action commencing unimpaired by the presence of extraneous salts, and its future efficacy undiminished by the progress of their removal,—that if this could be accomplished, there would be a far greater chance of ultimate success than has been offered by any plan yet made public.

"It is well known that silica can, by appropriate means, be obtained in the form of a pure aqueous solution, and it was to this liquid that we accordingly directed our attention. This solution can be made in several ways:

"1. By dissolving sulphide of silicium in water, when sulphuretted hydrogen is given off, and the silica remains completely dissolved, and in such quantity that the liquid gelatinizes when an attempt is made to evaporate it.

"2. By precipitating silica in the gelatinous state from an alkaline silicate, by means of acetic or other weak acid, and, after well washing, heating it for some time under pressure, with a small quantity of water in a closed vessel. A liquid is thus obtained which gelatinizes on addition of a saline solution.

"3. By passing gaseous fluoride of silicium over crystallized boracic acid, and separating the hydrofluoric and boracic acids by digestion with a large excess of ammonia, a hydrate of silica remains, which, when well washed from the above acids, is very soluble in water. This solution gives no precipitate when boiled, but leaves silica as an insoluble powder on evaporation.

"4. By the beautiful method recently pointed out by Professor Graham, in which advantage is taken of the new means of separating

bodies by *dialysis*. A solution of silicate of soda, supersaturated with hydrochloric acid, is placed on one side of a parchment paper septum, pure water being on the other side; in a few days the hydrochloric acid and chloride of sodium will be found to have completely passed through the diaphragm, leaving the silica in aqueous solution, and so pure that acid nitrate of silver fails to detect chlorine in the liquid. This solution remains fluid for some days, but it ultimately gelatinizes. We have generally adopted this last plan of preparing the aqueous solution of silica, although a stronger solution is obtained by the method first given.

"When a pure aqueous solution of silicic acid prepared as above is allowed to soak into the pores of chalk or dolomite, a process of hardening rapidly occurs, which goes on increasing for several days, whilst, owing to its considerable depth of penetration, and to there being no soluble or efflorescent compounds to be removed, there is every probability that this hard silicious impregnation will afford permanent protection to the stone. We are now actively engaged in investigating the nature of the action which takes place, and already several curious and important results have been made out, from which we are led to anticipate that our experiments will ultimately be rewarded with complete success."

It may not be uninteresting to the practical minds of the gentlemen present, to be informed of some of the uses of this *hydrated silica*. These uses amply show the reason why so many of the prominent European chemists have turned their attention to this subject, and why the English and French governments aid with means and protection the progress in this department of industry.

Some of the applications of hydrated silica.—1. For the preservation of building stones and decaying monuments. The Philadelphia Custom House (an important building in these times of "duties wanted"), and many other buildings in this city, are greatly in need of preservation.

2. For the formation of artificial building and other stones, by admixture of other cheap materials, such as lime, clay, sand, &c., in proper proportions to the liquid silica. Why, I ask, not build our fortifications with the aid of hydrated silica?

3. Wood, by its use, is most effectually rendered fire and water proof, and no decay can enter wood after it has been properly treated. Is it not likely that this article will become of great importance to us for coating ships, bridges, railroad cars, wooden houses, barns, roofs, and fences; for the impregnation of timber used for railroad sleepers, telegraph poles, wharves, &c.? Is our government aware of the value of this liquid?

4. The fresco paintings of Kaulbach, and other German, French, and English painters, have been executed with silicated colors. These paintings will, like the celebrated frescoes at Herculaneum and Pompeii (which chemical analysis shows to have been preserved for so many years by the use of silica colors), withstand the tooth of age. The insides of our public buildings, churches, mansions, and other structures, offer a large field to an army of artists.

5. As hydrated silica, when mixed with a properly prepared mate-

rial, forms a solid and durable marble, susceptible of the most beautiful and variegated coloration, the master-pieces of the art of sculpture *can be cast*, in the same way as the figures now made of plaster of Paris. Tombstones and similar monumental ornaments can thus be placed within the reach of the poorer classes of society. How much more attractive would our squares and public places look if they were tastefully ornamented with fine statuary and bas-reliefs!

6. Pure hydrated silica can, by substitution or otherwise, be made to combine with many organic bases, to form most beautiful and useful materials for gun carriages, wheels, tables, doors, furniture, telegraphic wire covering and insulators, philosophical instruments, parts of all kinds of machinery, hones, grindstones, razor strops, &c.

7. For coating metals to prevent oxidation and electric conduction.

8. The extraction of gold, silver, platina, and other metals, by simply dissolving the silicates and precipitating the metals by their own specific gravity, or by chemical or electrical means, will enable those engaged in this branch of business to procure all the metal from the ore, while, at the same time, they produce a soluble silicate of commercial value.

Many more applications could be enumerated; I think, however, that enough have been given to direct the attention of practical minds to this comparatively new yet highly important subject.

Mr. Fleury exhibited a fine specimen of Colored Marble, made by a patented process purchased by Mr. Waldron J. Cheyney, of this city.

Mr. Fleury also read the following paper on Imitation of Russia Sheet Iron:

The samples of iron, which I now have the pleasure of presenting to the Institute, were made under the patent of Mr. Wm. Riesz from ordinary rolled iron, the original cost of which was 5 cents per pound; the expense of the process was $2\frac{1}{2}$ cents, making the total cost of the iron in its present condition, $7\frac{1}{2}$ cents per pound.

The inventor, who was for a number of years director of a large iron manufacturing establishment in Germany, had made it his particular study to examine theoretically and practically the manufacture of the iron which was imported in large quantities from Russia. By repeated analyses of the iron, and also through noticing its beautiful, smooth, and incorrodible surface (by scraping off the surface from a large number of sheets), he came to the curious conclusion that the Russia iron was not, as he had thought, and as the general impression among iron manufacturers still seems to be, covered by *a film of carburet of iron*, but that the smooth surface consisted of *an atomic accumulation of a peculiar substance, a* NITRIDE *of iron combined with about* 20 *per cent. of carbon:* the nitro-carburetted iron of Fremy.* The quantity of carbon and nitrogen diminished gradually towards the centre, where the iron was nearly pure and very flexible. After years of experiments, he has finally succeeded in producing from ordinary sheet iron the best imitation of Russia sheet iron which, in my opinion, can be made.

* See London Chemical News, "On the Composition of Cast Iron and Steel," by M. E. Fremy, pp. 320, 331, 345, 361, 375. See also last number Journal of the Franklin Institute.

Before explaining the process and the interesting changes that take place, I will mention a few of the most important applications for this iron. The most extensive use of Russia iron is in the manufacture of stoves and stove pipes, where the black modification is preferred. The blue Russia iron we find applied in coverings of locomotive boilers, and other parts of steam machinery. The Massachusetts button manufacturers use black Russia iron for the bases of buttons, safe ornaments, door and other locks.

Large plates can be rolled together and welded to any size, then tempered and converted into steel. It is my opinion that such a plate of 2½ inches in thickness, would offer an equal resistance to the armor plates of the "*Black Warrior*," and I would suggest the use of such plates for the covering of our war vessels. Steel plates could be used for railroad springs, breastplates for soldiers, and for various other purposes. The inventor says that he can make this sheet iron into sword and knife steel. Corrugated sheet iron of this kind is most admirably adapted for the construction of railroad cars, army wagons, transportable rails, and army-boat skeletons, to be carried in pieces, and covered with rubber cloth when used, in a similar manner to life-boats; it may also be used for transportable houses, suitable for our western settlers and others similarly situated.

Though the process is very simple, it requires considerable skill, but once learned by short practice under the guidance of the inventor, it can be carried on in the most regular manner. The iron is cleaned in a sulphuric acid bath, then washed with an alkali and water, and placed in a peculiar mixture described in the patent, which prevents oxidation; it is then rolled with the before-named coating, and, after being re-heated, placed under the hammer to receive the required temper and smoothness.

When the black variety is heated, it becomes dark blue, while the white acquires a beautiful sky blue color. We thus see, that from one and the same iron, four beautifully colored varieties can be obtained.

BIBLIOGRAPHICAL NOTICE.

A Manual of Elementary Geometrical Drawing, involving three dimensions. By S. Edward Warren, C. E., Professor of Descriptive Geometry and Geometrical Drawing in the Rensselaer Polytechnic Institute, Troy, N. Y. New York, John Wiley, 1861. 12mo. pp. 105, plates 16.

This appears to be a valuable work upon a subject the importance of which our mechanics do not yet appear fully to appreciate. The objects of the work are well given in the preface, and proper importance is given to the necessity on the part of the draughtsman of understanding something of the nature of the object which he proposes to represent. The method of the book is good—the style terse and scientific; the definitions precise and easily understood; the illustrations well selected. The book is well printed, and creditable to its publisher.

METEOROLOGY.

For the Journal of the Franklin Institute.

The Meteorology of Philadelphia. By JAMES A. KIRKPATRICK, A.M.

NOVEMBER.—The temperature of November was about one degree below the average for the last eleven years, and more than two degrees below the temperature of November, 1860.

The warmest day of the month was the 2d, of which the mean temperature was 58·7°. Rain fell continuously on that day, and the temperature increased until it reached the maximum for the month, about eleven o'clock in the evening, when the thermometer indicated 63½°.

The coldest day was the 25th, with a mean temperature of 35½°. The register thermometer indicated the lowest degree (29) on two days, the 25th and the 27th.

The range of temperature for the month was, consequently, 34½°.

The temperature was at or below the freezing point on seven days of the month.

The greatest change of temperature in the course of a day was 19°, on the 1st; the least was 5°, on the 16th. The average daily oscillation of temperature for the month (13·08°) was very near the average for eleven years, but was a little more than one degree less than for November, 1860.

The greatest mean daily range of temperature was 10½°, and occurred between the 29th and 30th days of the month; the least was half a degree, and occurred between the last day of October and the first of November. The average daily range for the month (4·11°) was 1½° less than the average for eleven years, and about the same amount below that for November, 1860.

The greatest pressure of the atmosphere was 30·109 inches, and was observed at 7 A. M. on the 1st. The minimum pressure was 29·213 inches, and was observed at 2 P. M. on the 6th. The monthly range of pressure was 0·179 of an inch. The average pressure for the month (29·771 ins.) was two-hundredths of an inch less than that for November, 1860, and thirteen-hundredths of an inch less than the average for eleven years, which must be regarded as an anomaly.—According to received theories, the barometric pressure should be increasing, while in fact for this month, it was less than for any other November, during the whole period of observation. The nearest approach to it was 29·795 in November, 1860, and 29·797 in November, 1858.

The greatest mean daily range of atmospheric pressure was 0·513 of an inch, between the 1st and 2d days of the month; the least was 0·045 of an inch, between the 17th and 18th; and the average for the whole month, was 0·179 of an inch, which is very near the average for the whole period of observation.

The force of vapor and dew-point, were less than usual. The force of vapor was greatest (0·523in.) during the rain storm of the 2d of the month, and least (·099 in.) on the 19th.

The relative humidity was greatest on the morning of the 24th, and least on the afternoon of the 19th. The average for the month was very near the general average.

Rain or snow fell on eleven days of the month, to the depth of 4·613 inches, which was 1½ inches less than fell in November, 1860, but about one inch more than the average amount for the last eleven years.

The first *snow* of the season—but a few flakes—was observed on the 16th, but the first in any appreciable amount fell on the evening and night of the 24th. On the morning of the 25th it was half an inch deep on roofs and boards, though there was none on the pavements.

The first *ice* was observed in the streets of the city, on the morning of the 19th, two days earlier than last year.

There were but three days of the month entirely clear, or free from clouds at the hours of observation; and the sky was completely covered with clouds at those hours on four days of the month. The average amount of the sky covered with clouds both for the month of November, 1861, and for the same month for eleven years, was 57 per cent. of the visible sky.

A Comparison of some of the Meteorological Phenomena of NOVEMBER, 1861, *with those of* NOVEMBER, 1860, *and of the same month for* ELEVEN *years, at Philadelphia, Pa.* Latitude 39° 57½′ N.; longitude 75° 10½′ W. from Greenwich.

	Nov. 1861.	Nov. 1860.	Nov. 11 years.
Thermometer.—Highest,	63·5°	80°	80°
" Lowest,	29 0	16	16
" Daily oscillation,	13·08	14·43	13·36
" Mean daily range,	4·11	5·53	5·62
" Means at 7 A. M.,	39·85	43·30	41·10
" " 2 P. M.,	48·73	50·53	50·34
" " 9 P. M.,	43·25	45·33	44·34
" " for the month,	43·94	46·39	45·26
Barometer.—Highest,	30·109 in.	30·305 in	30·661 in.
" Lowest,	29·213	29·248	29·117
" Mean daily range,	·179	·197	·187
" Means at 7 A. M.,	29·793	29·821	29·922
" " 2 P. M.,	29·736	29·773	29 882
" " 9 P. M.,	29·785	29·792	29·908
" " for the month,	29·771	29·795	29·904
Force of Vapor.—Means at 7 A. M.,	·199 in.	·234 in.	·228 in.
" " " 2 P. M.,	·210	·228	·231
" " " 9 P. M.,	·206	·236	·235
" " " for the month,	·205	·233	·232
Relative Humidity.—Means at 7 A. M.,	78 per ct.	76 per ct.	78 per ct.
" " " 2 P. M.,	58	57	59
" " " 9 P. M.,	70	71	74
" " " for the month,	69	68	70
Rain and melted snow, amount	4·613 in.	6·057 in	3·765 in.
No. of days on which rain or snow fell,	11·	12	10·4
Prevailing winds—Times in 1000-ths,	x68°12′w.·324	x81°25′w ·333	x68°1′ w ·254